

# Essays in Applied Labor Economics

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presented by

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The Faculty of Economics, Business Administration and Information Technology of the University of Zurich hereby authorises the printing of this Doctoral Thesis, without thereby giving any opinion on the views contained therein.

Zurich, April 6, 2011

Chairman of the Doctoral Committee: Prof. Dr. Dieter Pfaff

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## Preface and Acknowledgements

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“I was working on the proof of one of my poems all the morning, and took out a comma. In the afternoon I put it back again.”

Oscar Wilde (1854 - 1900), Irish dramatist, novelist, and poet

I was first exposed to applied econometrics during my exchange year at the University of Lausanne, where a course in trade policy required me to write an empirical term paper. I ended up investigating whether the voting behavior of members of the U.S. House of Representatives on immigration issues is influenced by business and union contributions from industries which are heavily dependent on undocumented workers. The findings turned out to be mixed at best. Nonetheless, I have been fascinated by empirical work ever since.

As soon as I was back at the University of Zurich, I started focusing on econometric courses. Among them, Rafael Lalive’s course on causal analysis attracted my particular interest, a field only vaguely known to me at this time. This course has had a lasting impact on the way I think about applied econometrics: I shifted from approaching research questions by standard econometric methods towards the *quasi-experimental* approach (see Angrist and Pischke, 2009, for an outstanding text book about causal analysis).

Thanks to Josef Zweimüller and Rafael Lalive who employed me as research assistant at the chair for Macroeconomics at the University of Zurich while I was writing my Master’s thesis, I got the chance to gain valuable insights into the academic work environment. The inspiring and extremely pleasant working atmosphere Josef Zweimüller created (and still creates) at his chair dispelled any remaining doubts I had in deciding whether or not to start doctoral studies in the first place. This thesis is (hopefully) good evidence that I took the right decision.

It took me slightly more than four years to complete this work. During this time I greatly benefited from the help and support of many people. First and foremost, I would like to thank Josef Zweimüller and Rafael Lalive. Josef, my supervisor, was a constant source of research

ideas, and always helped me to stay focused on the most promising research projects as well as to tap their full potential. He is also the co-author of chapters 2, 3 and 5 in this thesis. Rafael's knowledge and experience in conducting sound and convincing empirical research was extremely enriching for me not only for my first dissertation paper he co-authored (presented in chapter 3), but also for all my subsequent research projects. I cannot emphasize enough how much I benefitted from both Josef's and Rafael's advice and support. Further, I am deeply grateful to Andreas Kuhn for being my main contact point for all sorts of questions (econometrics, economics, Stata,  $\text{\LaTeX}$ ), all of which he patiently answered. Moreover, I very much appreciated our fruitful collaboration on our joint project presented in chapter 2. I am also very grateful to Rainer Winkelmann for agreeing to be the co-advisor of my thesis.

Furthermore, I want to especially thank Christian Hepenstrick, with whom I had countless and very valuable discussions about economics, research, and being a doctoral student in general, for his pleasant and motivating companionship during the time we shared at the University of Zurich, at the Study Center Gerzensee, and in Cambridge. Special thanks also go to Holger Herz for the highly challenging time at the Study Center Gerzensee (problem sets, tennis, soccer, etc.) and also for the four years together at the University of Zurich. Further thanks go to Tobias Würzler for the good time and the delicious dinners (Thanksgiving!) at our shared flat in Jamaica Plain while being Graduate Visiting Students at the MIT, and Oliver Ruf and Simon Büchi for their support in working with the Austrian Social Security Database upon which chapters 2–5 are founded. I also want to thank Tanja Zehnder, Claudia Bernasconi, Beatrice Brunner, Andreas Kohler, Andreas Steinhauer, Simone Gaillard, Sandro Favre, Philippe Ruh, Jin Wiederkehr, Roger Abegg, and Katrin Koller, who all contributed to making my time at the Chair for Macroeconomics very enjoyable during coffee breaks, lunches, chair's excursions, and chair's "off-the-record"-excursions.

Last but not least, I would like to thank Eleonora (who also helped me to decide whether or not to put back any commas), my parents Ingrid and Joachim, and my sisters Véronique and Daniela for their kind support and encouragement throughout my dissertation. This book is dedicated to all of them.

Jean-Philippe Wüllrich, Cambridge (USA), October 2010

# CHAPTER 1

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## Introduction

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“Not everything that can be counted counts, and not everything that counts can be counted.”

Albert Einstein (1879 – 1955)

My thesis sheds empirical light on three research questions which belong to the realm of labor economics: What is the effect of early retirement on retirees’ mortality? How does an employment quota affect firms’ demand for disabled workers? And why are fluctuations in workplace accidents pro-cyclical? Chapter 2 shows that, for some groups of workers, early retirement increases the probability of premature death. Moreover, the driving force seems to be changes in health-related behavior, such as excessive alcohol consumption and smoking. The results in chapter 3 suggest that an employment quota is favorable to firms’ demand for disabled workers. In addition, chapter 4 finds that a rise in the tax, to which firms that do not comply with the employment quota are subject, also boosts this demand. Chapter 5 provides evidence that the pro-cyclicality of workplace accidents is governed by workers’ reporting behavior.

The two crucial ingredients required to provide the answers to these questions are adequate data and a convincing empirical strategy. All chapters work with the Austrian Social Security Database (see Zweimüller *et al.*, 2009, for a detailed description of this data set). This data set covers the universe of Austrian wage earners in the private sector and collects workers’ complete labor market and earnings history from 1972 onwards along with information about

socio-demographic characteristics and information about the employer. Moreover, these data can be linked to several other micro data sets (e.g. to causes-of-deaths statistics provided by Statistics Austria (see chapter 2), to individuals' disability status recorded by the Austrian Federal Welfare Office (see chapters 3 and 4), or to information on workplace accidents collected by the Austrian Social Insurance for Occupational Risks (see chapter 5)). The empirical strategy adopted in my thesis mainly comes in the flavor of the *quasi-experimental* approach (Meyer, 1995). Chapter 2 uses instrumental variable techniques (Angrist *et al.*, 1996; Imbens and Angrist, 1994), chapter 3 applies a regression discontinuity design (Hahn *et al.*, 2001), and chapter 4 an interrupted time series design (Cook and Campbell, 1979). Chapter 5, in contrast, uses standard panel data methods.

The availability of adequate data and the adoption of a convincing empirical strategy are clearly not sufficient for a research question to be of any relevance. Personally, I consider research questions relevant if their answers have implications also beyond academia. In my view, the ultimate aim of applied or empirical labor economics is to provide a sound basis upon which policy makers can build appropriate policy interventions in the labor market. This thesis seeks to live up to this ambition by stating clear policy implications that follow from my results. In other words, referring to the quote above, I hope that among those things that can be and are counted, which is – alas! – a necessary condition for carrying out empirical work, my research presented in the following four chapters does indeed count in the sense that it will eventually contribute to shaping better public policies. The remainder of this chapter provides a short overview on each of the four main chapters.

## **Chapter 2: Fatal Attraction? Access to Early Retirement and Mortality**

In many industrialized countries, dramatic demographic changes put governments under increasing pressure to implement major reforms to old age social security systems. A particular focus of many reforms is to increase the retirement age by restricting access to early retirement schemes. Chapter 2 of my thesis investigates whether attempts to raise the effective retirement age come along with consequences for the health of retirees. More precisely, we estimate the causal effect of early retirement on mortality for blue-collar workers. To overcome the problem of negative health selection, we exploit an exogenous change in unemployment insurance rules in Austria that allowed workers in eligible regions to withdraw permanently from employment up to 3.5 years earlier than workers in non-eligible regions. For males, instrumental-variable estimates show that retiring one year earlier causes a significant 2.4 percentage points (about 13%) increase in the probability of dying before age 67. We do not find any adverse effect of early retirement on mortality for females. Our analysis of death causes suggests that male excess mortality is concentrated among three causes of deaths: (i) ischemic heart diseases (mostly heart attacks), (ii) diseases related to excessive alcohol consumption, and (iii) vehicle



injuries. These causes account for 78 percent of the causal retirement effect (while accounting for only 24 percent of all deaths in the sample). About 32 percent of the causal retirement effect are directly attributable to smoking and excessive alcohol consumption.

Our results have an obvious policy implication. From a welfare point of view, our results suggest that early retirement has severe negative welfare consequences for male blue collar workers. Increasing the effective early retirement age is therefore warranted not only because it helps to resolve the financing problems of pay-as-you-go social security systems but also because it increases individual welfare. Increasing life expectancy by raising the effective retirement age, however, will not help to resolve the financing problem one-for-one because increases in life expectancy will partly offset the improvement in the worker-retiree ratio.

### **Chapter 3: Do Financial Incentives Affect Firms' Demand for Disabled Workers?**

Government efforts to eliminate employment discrimination of disabled workers are common in all OECD countries. However, previous literature has shown that e.g. anti-discrimination legislation does not necessarily improve economic conditions for the disabled (see Acemoglu and Angrist, 2001, for an extensive evaluation of such a legislation in the U.S.). A number of OECD countries use – on top of anti-discrimination legislation – employment quotas to counteract the wedge between disabled and non-disabled employment rates. For instance, Austrian firms must provide at least one job to a disabled worker per 25 non-disabled workers. Non-complying firms pay a tax for each job-month missed. Employment quotas have not been studied yet in the economic literature. Chapter 3 of my thesis fills this gap by studying the role of Austria's employment quota on firms' demand for disabled workers. Specifically, we compare firms who employ 25 non-disabled workers and are subject to the non-compliance tax to firms who employ 24 non-disabled and are not subject to the tax. While firms may manipulate non-disabled employment to avoid the tax, a simple framework suggests (i) manipulation introduces a downward bias of our estimates and (ii) manipulation need not be strong. Two important manipulation checks indeed support the claim that manipulation is unlikely to be quantitatively large in our context.

Results indicate that (i) firms with 25 non-disabled workers employ about 0.04 (or 12 percent) more disabled workers than would be expected from smaller firms, (ii) employment effects are stronger in low-wage firms than in high-wage firms, and (iii) the quota generates excess disabled employment on the order of 0.07 among firms located at non-disabled firm size 50 and higher. Two reforms of the system also suggest that increasing the non-compliance tax increases excess disabled employment, whereas paying a bonus to over-complying firms slightly dampens the employment effects of the non-compliance tax. These results suggest that employment quotas in favor of disabled workers are, in contrast to anti-discrimination legislation, an effective tool for promoting disabled employment.

## **Chapter 4: The Effects of Increasing Financial Incentives for Firms to Promote Employment of Disabled Workers**

Chapter 4 closely relates to chapter 3. It also investigates the employment quota in Austria, but focuses on the effect of an increase in the non-compliance tax from € 150 to € 196 in July 2001 on firms' demand for disabled workers. Adopting an interrupted time-series design, chapter 4 finds that this tax increase had an immediate as well as a short-run impact on the number of disabled workers at the firm level. By the end of 2002, one in 15 firms employ one disabled worker more than they would have without the tax increase. In terms of the average number of disabled workers, this corresponds to a 6.4% increase in the number of disabled workers per firm. I conclude that the tax increase considerably increased firms' demand for disabled workers and thus policy makers aiming at boosting employment of disabled workers should favor a further rise in the non-compliance tax.

## **Chapter 5: Recessions Are Bad for Workplace Safety**

Workplace accidents are an important economic phenomenon. Yet, the pro-cyclical fluctuations in workplace accidents are not well understood. They could be related to fluctuations in effort and working hours, but workplace accidents may also be affected by reporting behavior. Chapter 5 uses unique data on workplace accidents from an Austrian matched worker-firm dataset to study in detail how economic incentives affect workplace accidents. We find that workers who reported an accident in a particular period of time are more likely to be fired later on. Moreover, we find support for the idea that recessions influence the reporting of moderate workplace accidents: if workers think the probability of dismissals at the firm level is high, they are less likely to report a moderate workplace accident.

The cyclical sensitivity of the incidence of workplace accidents thus appears to be related to reporting behavior. As indicated in the theoretical part of this chapter, the cyclical fluctuations in reporting behavior has clear welfare implications as investments in prevention of workplace accidents may be suboptimal. If in recessions firing rates go up workers may under-report workplace accidents and thus firms under-invest in workplace safety. In booms workers may over-report workplace accidents and therefore firms over-invest in workplace safety, i.e. although workers benefit from the investments in workplace safety they are wasteful from a social point of view. Our empirical evidence suggests that recessions are bad for workplace safety. From the point of view of economic policy, a way to bring the economy closer to the social optimum would be to introduce measures that impede the discriminating layoffs of workers who reported an accident. This would increase the incentive of firms to invest in workplace safety also during recessions.

## CHAPTER 2

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### Fatal Attraction?

### Access to Early Retirement and Mortality

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Joint with Andreas Kuhn and Josef Zweimüller

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“Retirement kills more people than hard work ever did.”

Malcolm S. Forbes (1919 – 1990), publisher of Forbes magazine

## 2.1 Introduction

In many industrialized countries, dramatic demographic changes put governments under increasing pressure to implement major reforms to old age social security systems. A particular focus of many reforms is to increase the effective retirement age by restricting access to early retirement schemes. Workers and their political representatives often strongly oppose such reforms. Among the most important arguments is that, after having worked all their lives in physically demanding jobs, workers should have the option to retire early and thus avoid emerging health problems. While leaving an unhealthy work environment is, *ceteris paribus*,

clearly conducive to good health, the health effects of permanently exiting the labor force may go in the opposite direction. Retirement is not only associated with lower income and fewer resources to invest in one's health, but also with less cognitive and physical activity (Rohwedder and Willis, 2010) as well as with changes in daily routines and lifestyles which are potentially associated with unhealthy behavior (e.g. Balia and Jones, 2008; Henkens *et al.*, 2008; Midanik *et al.*, 1995; Scarmeas and Stern, 2003). In sum, the overall consequences of early retirement are not at all clear.

This study presents new evidence on the causal effect of early retirement on mortality for blue-collar workers. Blue-collar workers are an interesting group because they typically work in physically demanding jobs and because emerging health problems – and/or their prevention – often induce these workers to retire earlier. To solve the problem of negative health selection into retirement we take advantage of a major change to the Austrian unemployment insurance system which affected some but not all older workers. Defining the date of early retirement as the date of permanent withdrawal from employment, this policy change allowed older workers in eligible regions to retire up to 3.5 years earlier than comparable workers in non-eligible regions. Exploiting regional differences in eligibility to extended unemployment benefits of otherwise comparable workers allows us to overcome the reverse-causality problem. Since the program generates variation in the retirement age that is arguably exogenous to individuals' health status, we can estimate the causal impact of early retirement on mortality using instrumental variable techniques. Moreover, the comparison between OLS and IV estimates allows us to assess the extent of health-driven selection into early retirement.

We find that a reduction in the retirement age causes a significant increase in the risk of premature death – defined as death before age 67 – for males but not for females. The effect for males is not only statistically significant but also quantitatively important. According to our estimates, one additional year of early retirement causes an increase in the risk of premature death of 2.4 percentage points (a relative increase of about 13.4 percent). In line with expectations, we find that IV estimates are considerably smaller than the simple OLS estimate, both for men and for women. This is consistent with negative health selection into retirement and underlines the importance of a proper identification strategy when estimating the causal impact of early retirement on mortality. Our results indicate no causal effect of early retirement on mortality for females suggesting that the negative association between retirement age and mortality indicated by the OLS estimate is entirely due to negative health selection. There are several reasons why male but not female blue-collar workers suffer from higher mortality. Women may be more capable of coping with major life events such as retirement; they may be more health-conscious and adopt less unhealthy behaviors (such as smoking, drinking and unhealthy diet); they may be more active after permanently exiting the labor market due to their higher involvement in household activities; and they may suffer

less from a loss of social status and identity because work is less central in life for additional income earners as compared to the main breadwinner (our empirical analysis is based on older cohorts for whom the traditional role model is still the dominant one).

We consider several channels to understand why male early retirees die earlier. A *first* channel suggests that early exit from the labor market is associated with lower permanent income. We find that earnings losses due to early retirement cannot explain our finding for men, because these losses are quantitatively too small to have a substantial impact on mortality. A *second* channel suggests that changes in health-related behaviors associated with smoking, drinking, unhealthy diet, and little physical exercise may cause premature death following early retirement. Our results strongly support this hypothesis. We find that excess mortality is concentrated on three causes of deaths: (i) ischemic heart diseases (mostly heart attacks), (ii) diseases related to excessive alcohol consumption, and (iii) vehicle injuries. These three causes of death account for 78 percent of the causal retirement effect (while accounting for only 24 percent of all deaths in the sample). We calculate that 32.4 percent of the causal retirement effect can be directly attributed to smoking and excessive alcohol consumption. A *third* channel suggests that the detrimental mortality effect arises from retirement following an involuntary job loss but not from voluntary quits. Even though our data do not distinguish between voluntary and involuntary retirement, we exploit severance payment rules to proxy the voluntariness of the retirement decision. Our empirical results suggest that retirement following an involuntary job loss is likely to cause excess mortality among blue collar males, while retirement after a voluntary quit does not.

Our study goes beyond the existing literature in several respects. *First*, our empirical strategy is based upon a policy change that, arguably, generates huge exogenous variation in the potential minimum age of permanently leaving the labor force. While treated and control groups are ex-ante similar in observable characteristics, the group of eligible individuals retires between 9 and 12 months earlier than the group of non-eligible individuals. *Second*, we use an administrative data set containing precise and reliable information on both the timing of retirement and the date of death. Austrian social security data are collected for the purpose of assessing individuals' eligibility to (and level of) old age social security benefits. Information on any individual's work history and the date of his or her death is thus precise so our estimates are unlikely contaminated by measurement error. This is different from many previous studies which focused on subjective measures of health or well-being that are subject to non-negligible measurement problems.<sup>1</sup> *Third*, the data contains the universe of blue-collar workers in the

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<sup>1</sup>The distinction between subjective and objective measures appears to be of special importance (Bound, 1991), as even self-reported measures of physical health may be subject to considerable reporting error (Baker *et al.*, 2004). It is likely that truly subjective measures of health, i.e. individuals' assessment of their well-being, perform even worse because of ex-post justification bias and similar effects. Indeed, studies using subjective health measures tend to find beneficial effects of retirement while the evidence is less consistent for objective health measures. It is also conceivable that there is considerable measurement error with respect to retirement

private sector in Austria. Hence there is a sufficiently large number of observations that help us to get precise estimates. This is a particular advantage in the present context, because many previous studies (mostly those based on survey data) often face the problem of imprecise estimates due to small sample sizes.

While our empirical design is based on a policy change in a small country, we think our results are of more general interest. *First*, the effect we estimate with our empirical design is unlikely to originate from the particular institutional framework. Treated and control workers are both covered by mandatory universal health insurance and by a generous old-age social security system (for workers with a continuous employment history). This implies that our estimated effect cannot be driven by (lack of) access to health care or by major income losses after retirement. Instead we estimate a more direct effect of early retirement on mortality. In environments where retirees have no access to health care or suffer from major income losses after retirement, our estimate provides a lower bound. A *second* reason why we think that Austria is an interesting case is that early retirement is a very common phenomenon. In the early 1990s, the average age at retirement entry was as low as 58 for the whole Austrian population and it was even lower for blue collar workers. Hence the typical early retiree in our sample is quite similar (though clearly not identical) to the average blue collar worker rather than a member of a highly selective group.

Among the large number of papers studying the health and mortality effects of retirement, studies adopting convincing empirical strategies to estimate the causal impact of retirement on health and/or mortality are rare. Bound and Waidmann (2007) use institutional rules governing eligibility to public pensions to identify the effect of retirement on both subjective and objective measures of physical health, by relying both on survey data and vital statistics for the UK. They find no effects, or a slightly positive influence, of retirement on health, once the possibility of endogenous entry into retirement is taken into account. Even though institutional rules offer an apparently plausible instrument for the age at retirement, the fact that workers know the exact rules may render these instruments invalid.<sup>2</sup> Coe and Lindeboom (2008) improve on this methodology and exploit sudden and arguably unexpected changes in retirement opportunities (i.e. early retirement opportunities offered by firms to groups of workers) in the US to identify the causal effect of early retirement on men's health. They find no detrimental effects of early retirement on health and, if anything, even slightly temporary improvements.<sup>3</sup> Charles (2002) also uses age discontinuities in the financial incentives to

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age, especially in survey data, whereas such error is arguably of minor importance in administrative data.

<sup>2</sup>As pointed out by Coe and Lindeboom (2008), workers who know the exact legal rules may adjust their behavior before actually retiring. Moreover, workers subject to different retirement rules may also differ with respect to unobserved variables, absent any behavioral responses.

<sup>3</sup>A potential problem with this approach is that even though firms were restricted in targeting specific groups of individuals, they were free to choose whether or not to offer any early retirement window at all. Hence workers who were offered any early retirement opportunity may differ from workers who were not.



retire, as well as legal changes to these incentives, to identify the causal effect of retirement on subjective well-being. He finds a positive effect of retirement on subjective well-being when accounting for the endogeneity of the retirement decision, while the raw correlation between age at retirement and well-being is negative. Similar results on mental well-being are reported in Neuman (2008) for the US and Johnston and Lee (2009) for the UK, and Coe and Zamarro (2008) in a cross-country study for Europe, all using survey data and a similar empirical design. Kerkhofs and Lindeboom (1997) use panel-data methods to study the effects of labor market status on the health of Dutch elderly, finding that early retirement has a positive impact on self-assessed measures of health. One of the few studies finding a detrimental effect of retirement on health is Behncke (2009), who applies matching methods to survey data from the UK. She finds that retirement increases both the risk of a cardiovascular disease and the risk of being diagnosed with cancer. While the estimated positive effect on several health outcomes is in line with much of the medical literature (see also footnote 4), the empirical design may still suffer from endogeneity bias due to unobserved factors (such as individuals' true health status). Qualitatively similar results are reported in Dave *et al.* (2008), who analyze the effects of retirement using panel-data methods and relying on survey data from the US. They find negative effects of retirement on both mental health and measures of self-assessed physical health. Note, however, that conventional panel-data methods are vulnerable to time-varying unobserved confounders such as unobserved health shocks. In sum, the available evidence uses different outcome measures and different strategies to deal with endogenous entry into retirement and, consequently, yields no clear pattern regarding the causal impact of retirement on health.<sup>4</sup>

Our study is also related to a literature that focuses on the impact of involuntary job loss on mortality, with respect to both research topic and methodology. An interesting recent study by Sullivan and Wachter (2009) estimates the effect of job displacement on mortality in the US. They find a strong impact of involuntary job loss on mortality, particularly for older (high-seniority) workers and for workers who suffer large earnings losses (i.e. low-wage workers). In a related study in Sweden, Eliason and Storrie (2009) examine the impact of job loss on cause-specific mortality. They find a strong increase in overall mortality among men, but no impact on females. There was, however, an increase in suicides and alcohol-related mortality for both men and women. Adverse effects of involuntary job loss on mortality are

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<sup>4</sup>Unsurprisingly, similar ambivalence regarding the health effects of retirement is found among medical and epidemiological studies. Bamia *et al.* (2008) find that the risk of all-cause mortality is significantly higher for retirees than for older workers still engaged in economic activity. This finding is consistent with the results of Gallo *et al.* (2006), who argue that job loss increases individuals' risk of cardiovascular disease and therefore has detrimental effects on the health of older workers. Morris *et al.* (1994) also find increases risk of cardiovascular disease for the UK. Somewhat contrasting evidence is presented in Tsai *et al.* (2005) who study the effects of early retirement on mortality in a very specific sample of workers in the petrochemical industry. Similarly, Litwin (2007) finds no association between early retirement and all-cause mortality and Brockmann *et al.* (2009) find no effect of retirement on health, at least when focusing on previously healthy workers only.

also reported in another recent study based on Norwegian data by Rege *et al.* (2009).

The chapter is structured as follows. In section 2.2 we discuss the institutional background and we describe how changes in the unemployment insurance system lead to early permanent withdrawal from work for some groups of workers. Section 2.3 discusses the data source as well as the selection of our sample and presents descriptive statistics. Details of our econometric framework are given in section 2.4. Our results are presented in sections 2.5 and 2.6. In section 2.7, we focus on potential channels explaining excess mortality among male retirees. Section 2.8 concludes.

## 2.2 Pathways to Retirement in Austria

In this section we describe the various pathways into early retirement in Austria. We define as “early retirement” the date at which an individual withdraws permanently from the labor market. This does not require the individual to be a retiree in the legal sense of drawing regular old age social security benefits. Instead, our definition of early retirement hinges upon the last day of regular employment and does not refer to the particular transfer an individual gets after having permanently withdrawn from work.

### 2.2.1 The Retirement System

Almost all workers in Austria are covered by the old age social security system, and the benefits paid by this system are the most important source of income for retirees (OECD, 2007). The level of old age social security benefits depends on retirement age, the contributions (i.e. earnings) made to the system in the years before retirement as well as on the number of contribution months (i.e. work experience).<sup>5</sup> The maximum gross replacement rate for a worker retiring at the statutory retirement age in the year 1993 was 80% of his or her previous earnings, given a continuous work history with 45 insurance years before retiring. Social security benefits are subject to income tax and mandatory health insurance contributions. The regular statutory retirement age is age 65 for men and age 60 for women. For workers with long-insurance duration the statutory retirement age is age 60 for men and age 55 for women (“vorzeitige Alterspension wegen langer Versicherungsdauer”). Eligibility to statutory retirement with long-insurance duration is linked to an individual’s previous work history: workers who paid social security contributions for at least 35 years and who worked at least 2 out of the 3 years prior to retirement have the option to retire early at age 60 for men and at age 55 for women.

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<sup>5</sup>There were several changes to the pension system during our observation period. However, these changes affected both the treatment and the control group in the same way. See Hofer and Koman (2006) for details.



There are several pathways into regular retirement. A first pathway is the direct transition from employment to retirement. A second pathway is the indirect transition from employment to retirement via the unemployment system. Individuals with a continuous work history become eligible for regular old-age social security benefits at age 60 after having drawn regular unemployment benefits and/or means-tested unemployment assistance for at least 12 out of the previous 15 months (“vorzeitige Alterspension wegen Arbeitslosigkeit”). An unemployed person aged 50 or older could draw regular unemployment benefits for a maximum period of 52 weeks (30 weeks before August 1989) with a replacement rate of 40–60%. Unemployment assistance payments may, in principle, last for an indefinite time period. Alternatively, unemployed individuals who had paid social security contributions for at least 15 out of the last 25 years are also eligible to regular early retirement benefits at age 60 after a period of 12 months in special income support (“Sonderunterstützung”), which is equivalent to a regular unemployment spell but grants a transfer that is 25% higher than regular unemployment benefits. Individuals eligible to special income support could “move” from unemployment benefits to special income support. This pathway essentially allowed workers to withdraw permanently from work at age 58 and bridge the gap to regular old age social-security benefits via an unemployment spell of 52 weeks (30 weeks before August 1989) and special income support for another 12 months. A third pathway is via disability insurance. This latter pathway becomes more easy to access after age 55 when eligibility rules to disability benefits become significantly relaxed (Hofer and Koman, 2006).<sup>6</sup>

### 2.2.2 The Regional Extended Benefit Program

To assess the causal effect of early retirement on mortality, we exploit a policy change to the Austrian unemployment insurance system that introduced a further pathway to retirement, the Regional Extended Benefit Program (REBP). The REBP allowed eligible workers to withdraw permanently from employment as much as 3.5 years earlier than non-eligible workers. The REBP was introduced in response to the steel crisis of the late 1980s which hit certain regions of the country particularly hard. To mitigate economic hardship in these regions, the Austrian government enacted a change in the unemployment insurance law that granted access to unemployment benefits (UB) for up to 209 weeks.<sup>7</sup> To become eligible, a worker had to fulfill the following three criteria at the time of unemployment entry: (i) age 50 or older, (ii) a continuous work history before becoming unemployed (i.e. 780 weeks of employment in the

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<sup>6</sup>After age 55, disability benefits could be drawn when an individual’s work capacity within his or her main occupation is reduced by more than 50 percent of that of a healthy individual. Before age 55, a reduction of the individual’s general work capacity, not restricted to a particular occupation, is required for eligibility to a disability pension.

<sup>7</sup>Previous econometric evaluations of the REBP have found large effects of the program on realized unemployment duration (Lalive, 2008a; Lalive and Zweimüller, 2004a,b; Winter-Ebmer, 1998).

last 25 years preceding the unemployment spell), and (iii) at least 6 months of residence in one of the eligible regions. The program was enacted in June 1988 and remained in force until July 1993.<sup>8</sup> In contrast, workers aged 50 or older who were not eligible to the REBP were entitled to a maximum of 52 weeks of regular unemployment benefits (to only 30 weeks before August 1989).

Figure 2.1 summarizes the institutional design of our study. The figure makes clear that individuals eligible to the REBP could effectively withdraw from the labor force at age 55 (men) or 50 (women) by claiming unemployment benefits for the maximum duration of 4 years, followed by one full year of special income support. This is different for workers not eligible to the REBP. Male workers had the option of effective retirement at age 58 (58.5 before August 1989) and female workers at age 53 (53.5 before August 1989) by bridging the time until the regular early retirement age by exhausting the maximum duration of unemployment benefits of 52 weeks (30 weeks before August 1989) followed by a year of special income support.

## 2.3 Data and Sample

### 2.3.1 Data Source

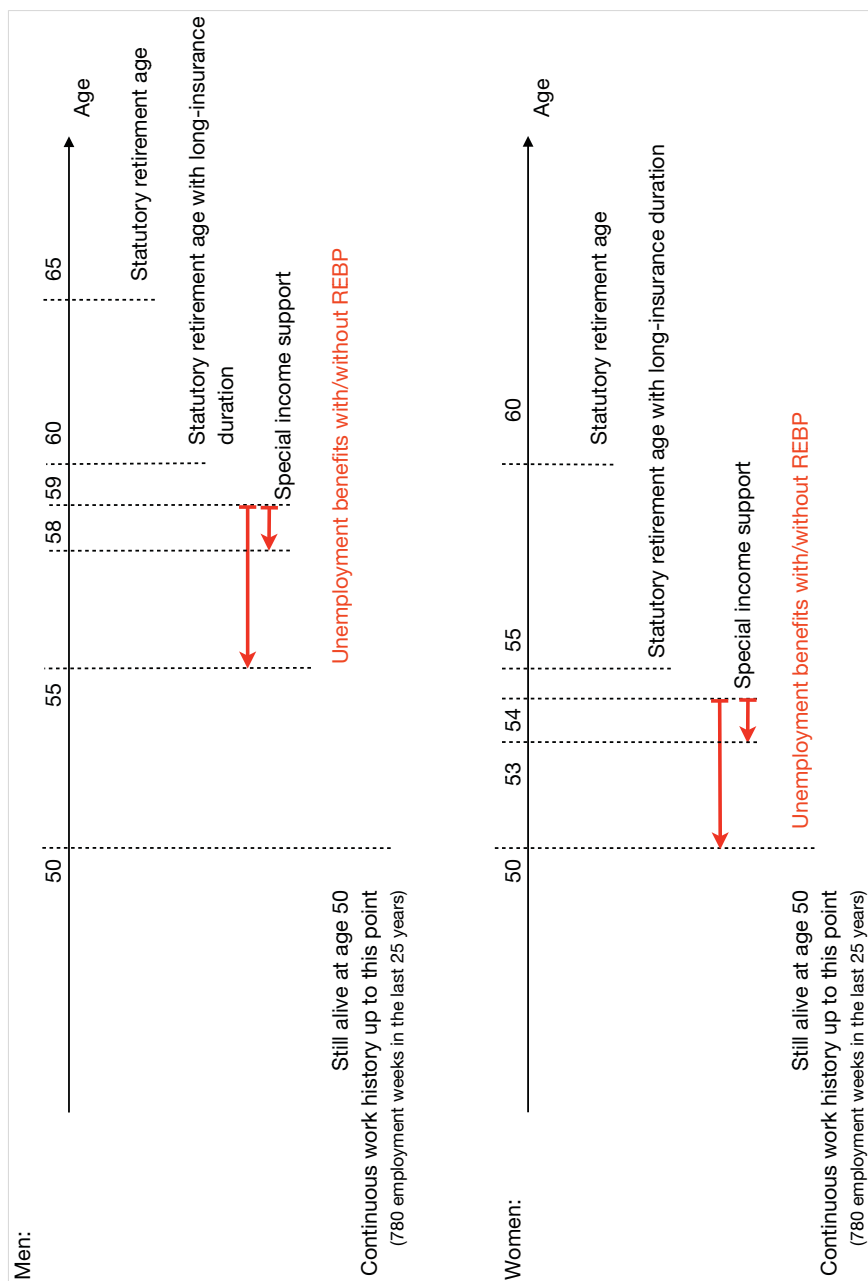
We use individual register data from the Austrian Social Security Database (ASSD), described in more detail in Zweimüller *et al.* (2009). The data cover the universe of Austrian wage earners in the private sector and collects, on a daily basis, workers' complete labor market and earnings history up to the year 2006. The data also contain a limited set of socio-economic characteristics (year and month of birth, age, sex, general occupation) and additional information on the firms where the workers were employed. The administrative purpose of collecting these data is to provide all the information necessary for calculating old age social security benefits.

The data contain precise information on the date of retirement and on mortality (date of death). Information on mortality is observable up to the year 2008. Moreover, the data contain information necessary for determining an individual's eligibility to the REBP. This latter information is of crucial importance because we want to exploit the exogenous variation in the retirement age that the program induces, i.e. we will use eligibility status as an instrument for the retirement age. We use information on individuals' month of birth and employment history to determine whether a worker meets the age and employment criteria set by the

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<sup>8</sup> Initially 28 out of about 100 labor market districts were eligible to extended unemployment benefits. The REBP underwent a reform in January 1992 that excluded 6 formerly eligible regions from the program. Moreover, eligibility criteria were tightened, as not only location of residence but also the individual's workplace had to be in a REBP region (see section 2.3.2 for details).

Figure 2.1: Earliest possible withdrawal from the labor force



REBP. However, we do not observe the place of residence. To proxy community of residence we use the community of work. While this introduces some measurement error due to the false classification of REBP eligible workers as non-eligible and vice versa, we find that this is not a major drawback, as most individuals work in the same labor market district where they live.<sup>9</sup>

### 2.3.2 Sample Selection

#### Workers

First, we restrict the analysis to blue collar workers.<sup>10</sup> The main reason for our focus on blue collar workers is that the REBP was a program targeted towards regions with a high dominance of blue collar workers. While the program was, in principle, also available to white collar workers, effective take-up by white collars was weak.<sup>11</sup>

We further restrict the sample to workers who meet the age criteria at some time during the period the REBP was in effect and who had a continuous work history before reaching the age of 50. The *age criterion* implies that we consider only men born between July 1929 and December 1941 and women born between July 1934 and December 1941, respectively.<sup>12</sup> This ensures that these individuals eventually attain age 50 during the REBP and men (women) are aged 59 (54) or less when the program was introduced. Put differently, while these cohorts were able to retire earlier (recall that men/women can claim special income support as soon as they turn 59/54) not all of them could take full advantage of the program. For instance, males born between 1934 and 1938 could take full advantage of the REBP because they reached age 55 during the time the REBP was in place. In contrast, males born before 1934 were too old to take full advantage (i.e. they already were 56 years old when the REBP started) and

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<sup>9</sup>We can check the extent of measurement error introduced by this proxy since we can observe the place of residence for individuals on unemployment benefits. We correctly assess REBP-eligibility for more than 90% of all individuals in this subsample if place of work instead of place of residence is used to assess REBP eligibility.

<sup>10</sup>Because blue and white collar workers in Austria are partially subject to different social security rules (for example, there are differences in notice periods and the duration of sick leave benefits), we can determine workers' occupational status without any significant measurement error.

<sup>11</sup>In fact, eligibility status is a highly significant predictor of early retirement among blue collar workers, but not among white collar workers. One potential explanation is that blue collar (low income) workers face higher replacement rates than white collar (higher income) workers when unemployed and thus higher incentives for taking advantage of the program. Specifically, replacement rates (both with respect to unemployment benefits and early retirement benefits) are much lower for white collar workers due to earnings caps. Because the instrument is too weak, results remain inconclusive in the case of white collar workers (results for white collar workers are available upon request).

<sup>12</sup>In principle, we could also consider the cohorts born from January 1942 to July 1943 as they (eventually) meet the age criteria as well. However, the data available to us from the ASSD only tracks individuals' labor market histories up to 2006. We omit cohorts born later than December 1941 in order to observe individuals' labor market histories at least until age 65 (i.e. men's statutory retirement age).

cohorts born after 1939 were too young (i.e. they were younger than 55 when the REBP was abolished).

The *experience criterion* selects workers who meet the REBP work experience requirement, i.e. workers with at least 15 employment years during the last 25 years. Furthermore, we only consider individuals with at least one employment year during the last two years at age 50, a requirement for being eligible to draw unemployment benefits. Because all selected individuals meet both the age and the experience criteria, the assessment of whether or not a worker is eligible to extended UB entitlement entirely hinges on individuals' place of residence (proxied by place of work; see footnote 9). This means that by using REBP eligibility as instrument for the retirement age, we basically compare individuals who work in eligible districts with those who work in non-eligible districts (section 2.4 provides the details).

Finally, we drop workers from the steel sector because our instrument does not induce changes in the retirement age for these workers. The reason is that, apart from the REBP, there was a second important program to alleviate problems associated with mass redundancies in the steel sector, the "steel foundation". This program was available both in treated and in control regions. Firms in the steel sector could decide whether to join, in order to provide their displaced workers with state-subsidized re-training measures organized by the foundation. Member firms had to co-finance this foundation. Displaced individuals who decided to join this outplacement center were entitled to claim regular unemployment benefits for a period of up to 3 years (later 4 years), regardless of age and place of residence (see Winter-Ebmer, 2001, for an evaluation of the steel foundation). We therefore do not find any difference in the retirement age between steel-workers in eligible and non-eligible regions.

## Regions

To make sure that potential differences in labor market conditions between treated and control regions do not contaminate our empirical estimates, we contrast only those eligible and non-eligible districts that are adjacent to each other and economically similar. We use the common classification of territorial units for statistics (NUTS). NUTS comes in three aggregation levels, of which we choose the most disaggregated one (NUTS-3).<sup>13</sup> We further confine our sample to those NUTS-3 regions that contain both eligible and non-eligible districts. Since NUTS-3 regions comprise geographically adjacent districts and because these units are quite small, this procedure implies that differences in labor market conditions between treated and control

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<sup>13</sup>NUTS-3 units are defined in terms of the existing administrative units in the EU member states. An administrative unit corresponds to a geographical area for which an administrative authority has power to take administrative or policy decisions in accordance with the legal and institutional framework of the member state. There are 35 distinct NUTS-3 units in Austria, each consisting of one or more district(s) ("Bezirk(e)").

regions are unlikely to affect our analysis.<sup>14</sup>

Figure 2.2 highlights the communities within those eight NUTS-3 units that we actually consider in the empirical analysis. The areas in black denote eligible communities and the areas in dark gray denote non-eligible communities within these NUTS-3 units, respectively. The remaining communities, i.e. those shaded in light gray, denote eligible and non-eligible communities which are not considered in the analysis.

### 2.3.3 Key Measures

The key variables of our analysis are our measures of early retirement and mortality. As mentioned above our sample includes only cohorts born between 1929 and 1941 (men) and 1929 and 1934 (women), respectively. Because information on labor-market histories is only available until December 2006 and information on mortality only until July 2008, individual labor-market histories of workers included in the sample can be tracked (at least) up to age 65 and individuals' mortality-related information is available (at least) up to age 67. We use this to define our dependent variable indicating premature death, a dummy variable that indicates whether a worker died before reaching age 67.<sup>15</sup>

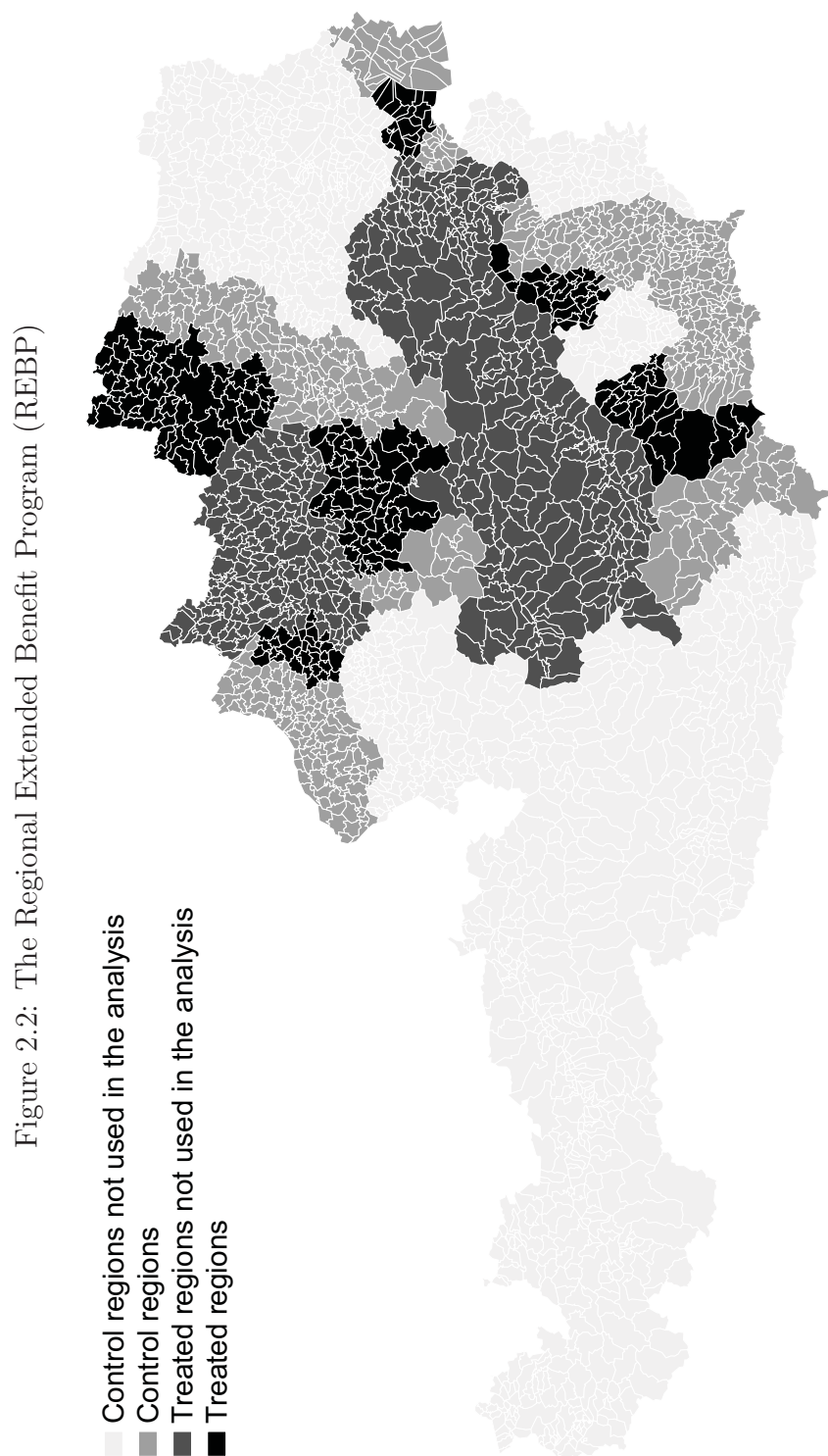
Since workers in our sample have to be alive at age 50 and meet the REBP age and experience criteria. Hence our mortality indicator measures whether or not an individual in our sample dies between age 50 and age 67. This is a meaningful indicator in the present context. Since we are studying birth cohorts 1941 and older, we are considering individuals whose life expectancy is still quite low (see footnote 15). Moreover, we look at blue-collar workers whose life expectancy is lower than that of white-collar workers. In our sample, the probability of death before age 67 is 18.0 percent for males and 7.2 percent for females.

Our treatment variable is the number of early retirement years. This variable measures the time span between the statutory retirement age at age 65 (for men) and 60 (for women), respectively, and the date when the individual permanently withdraws from working life. More

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<sup>14</sup>However, the map also shows that treated regions were not selected randomly. Even though we think that there is no strong a-priori reason for believing that individuals' health status was decisive in determining a given community's treatment status, we will return to this issue later (see section 2.4 below). See also the discussion in Winter-Ebmer (1998) and Lalive and Zweimüller (2004a,b) on how the regions were selected for eligibility in the first place. Importantly, Lalive and Zweimüller (2004a) show that both employment and unemployment rates for (potentially) eligible workers were quite similar before the start of the program. However, they also show that the program significantly increased the risk of unemployment for older workers, suggesting that the program may have been used deliberately as a path into early retirement, especially for women (Lalive, 2008a). Indeed, our results on the first-stage effect of the program are perfectly in line with this finding (see section ?? below).

<sup>15</sup>One might object that this measure is ill-suited for studying mortality because it only covers deaths occurring between age 50 and age 67. Note, however, that life expectancy at birth was not yet very high for those birth cohorts considered in the analysis. In fact, according to the life table based on data from 1930/33, life expectancy at birth (at age 45) was 54.5 (24.7) years for men and 58.5 (27.0) years for women (figures taken from Statistics Austria).



Notes: The figure shows which communities were, or were not, eligible to the REBP. Communities shaded in dark gray (light gray) were (were not) eligible to the REBP. Darker (lighter) shaded regions denote communities which we use (do not use) in the empirical analysis.



precisely, we define the date of retirement as the day after the end of the individual's last regular employment spell.<sup>16</sup> Hence a positive number on the endogenous variable denotes that an individual has retired before the statutory retirement age. Throughout the analysis, we will stratify the sample by gender because male and female retirement and mortality patterns are very different.

### 2.3.4 Descriptive Statistics

Table 2.1 shows descriptive statistics for our two different subsamples and by eligibility status. Our sample consists of 17,590 blue-collar males and 3,283 blue-collar females of whom 18.0 percent and 7.2 percent die before age of 67, respectively. Male workers in eligible districts retire 0.75 years (9 months) earlier than their colleagues in non-eligible regions. This is strong prima-facie evidence that male workers use the REBP as an indirect channel into early retirement. The situation is even more pronounced for females, who retire 1.15 years (14 months) earlier in treated than in control regions.

Table 2.1 also shows that the treated and control samples are well balanced (though not identical) with respect to observable characteristics. Columns (1) to (4) shows almost no difference in average (and variance of) age, indicating the absence of any major differences in age composition of the blue collar workers between the two types of regions. The various variables describing the previous work experience indicate slightly higher work experience before age 50 in non-eligible regions; the difference is rather small, however. Interestingly, blue collar workers in eligible regions were slightly less often on sick leave before age 50 than workers in control regions. Moreover, male blue collar workers in treated regions earned higher wages before age 50 (average earnings at ages 43 to 49) than those in control regions. We also see that the industry mix between regions is similar though not identical. There is a somewhat higher fraction of manufacturing workers in treated regions, and a somewhat larger fraction of construction and agriculture workers in control regions. Since treated and control groups are similar but not identical controlling for remaining differences in worker characteristics and in industry structure is potentially important in the empirical analysis below.

Columns (5) to (8) show analogous descriptive statistics for female blue collar workers. It turns out that the differences across regions among females are very similar to those among males. There is only a negligible difference in age and experience indicators. Blue collar females in treated regions have a lower incidence of sick days, earn somewhat higher wages, and are more concentrated in manufacturing than blue-collar females in control regions.

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<sup>16</sup>Recall that our indicator does not require the individual to be a retiree in the legal sense of drawing regular old age social security benefits. Instead, our definition of effective retirement hinges upon the last day of employment and does not refer to a particular transfer an individual gets after ceasing work permanently. Effectively retired individuals draw unemployment benefits, disability benefits, old-age social security benefits, some other type of benefit, or no transfer.



Table 2.1: Summary statistics

	Men			Women		
	Eligible districts	Non-eligible districts		Eligible districts	Non-eligible districts	
Retirement age	55.9411	(2.9171)	56.6912	(2.9046)	52.9410	(2.2170)
Retirement years before statutory retirement age	9.0589	(2.9171)	8.3088	(2.9046)	7.0590	(2.2170)
Age on June 1, 1988	52.8384	(3.7220)	52.7136	(3.7584)	50.0198	(2.1792)
Work experience before age 50 (in years)						
Within the last year	0.9623	(0.1102)	0.9490	(0.1217)	0.9598	(0.1303)
Within the last 2 years	1.9290	(0.1727)	1.9038	(0.1949)	1.9362	(0.1640)
Within the last 5 years	4.8163	(0.4108)	4.7572	(0.4616)	4.8302	(0.3794)
Within the last 10 years	9.6263	(0.7444)	9.5108	(0.8410)	9.5507	(0.8224)
Within the last 25 years	23.8921	(1.7633)	23.4873	(2.0540)	21.1592	(3.1414)
Number of sick leave days						
Within the last year	4.6800	(21.9293)	5.3438	(23.2272)	6.4036	(25.8762)
Within the last 2 years (in days)	7.7357	(29.1986)	8.4643	(30.2857)	10.1690	(33.7910)
Within the last 5 years (in days)	15.9685	(43.5918)	17.8007	(47.6949)	19.7249	(50.2334)
Within the last 10 years (in days)	38.3429	(70.4117)	40.1225	(74.8618)	31.8233	(64.5720)
Log(average yearly earnings (age 43-49))	9.9181	(0.2968)	9.8219	(0.2995)	9.4013	(0.3885)
Log(std. dev. of yearly earnings (age 43-49))	7.3430	(0.6966)	7.3534	(0.7363)	7.0143	(0.8072)
Industry affiliation (dummy indicators)						
Agriculture, fishery, forestry	0.0576	(0.2330)	0.0898	(0.2860)	0.0283	(0.1658)
Electricity, gas, heat, and water supply	0.0078	(0.0882)	0.0102	(0.1007)	0.0032	(0.0566)
Mining	0.0820	(0.2743)	0.0519	(0.2218)	0.0084	(0.0911)
Manufacturing	0.5793	(0.4937)	0.4468	(0.4972)	0.7404	(0.4386)
Construction	0.1600	(0.3666)	0.2414	(0.4280)	0.0174	(0.1306)
Retail, wholesale, stockkeeping	0.0571	(0.2321)	0.0851	(0.2790)	0.0514	(0.2209)
Tourism	0.0056	(0.0745)	0.0087	(0.0930)	0.0450	(0.2073)
Transport	0.0327	(0.1778)	0.0441	(0.2052)	0.0019	(0.0439)
Financial services, insurance	0.0081	(0.0895)	0.0102	(0.1007)	0.0135	(0.1154)
Personal hygiene	0.0030	(0.0544)	0.0031	(0.0552)	0.0212	(0.1441)
Arts, entertainment, sports	0.0010	(0.0308)	0.0001	(0.0104)	0.0013	(0.0358)
Health care	0.0051	(0.0713)	0.0064	(0.0800)	0.0617	(0.2407)
Educational system, research industry	0.0007	(0.0267)	0.0015	(0.0390)	0.0045	(0.0669)
Domestic servicing and maintenance	0.0001	(0.0109)	0.0005	(0.0233)	0.0019	(0.0439)
NUTS-3 units (dummy indicators)						
Nordburgenland	0.0259	(0.1588)	0.0750	(0.2634)	0.0437	(0.2045)
Mostviertel-Eisenwurzen	0.1290	(0.3352)	0.1403	(0.3474)	0.1298	(0.3362)
Waldviertel	0.1936	(0.3952)	0.1142	(0.3180)	0.3927	(0.4885)
Unterkärnten	0.0742	(0.2622)	0.1361	(0.3429)	0.0476	(0.2129)
Oststeiermark	0.1013	(0.3018)	0.1343	(0.3410)	0.0598	(0.2371)
West- und Suedsteiermark	0.1294	(0.3356)	0.1091	(0.3118)	0.0411	(0.1987)
Innviertel	0.0544	(0.2268)	0.2178	(0.4127)	0.0566	(0.2311)
Steyr-Kirchdorf	0.2922	(0.4548)	0.0732	(0.2604)	0.2288	(0.4202)
Number of observations	8,419		9,171		1,556	
						1,727

Notes: Sample means and standard deviations (in parentheses).

## 2.4 Econometric Framework

Estimating the causal effect of early retirement on health and mortality is difficult because poor health is a key determinant in individuals' retirement decisions (e.g. Disney *et al.*, 2006; Dwyer and Mitchell, 1999). This negative health selection implies that simple OLS estimates of a regression of individuals' mortality risk on an indicator of early retirement will overestimate the true causal effect of early retirement on mortality. We now detail how we deal with this issue.

To fix ideas, let  $Death_i^{67}$  denote a dummy variable indicating death before age 67 (such that  $Y_i$  takes on the value 1 in the event of death before age 67, and 0 otherwise) and let  $ER_i$  denote the number of years spent in early retirement. That is,  $ER_i$  measures the difference between the statutory and actual retirement age such that positive values correspond to exit from the labor force before the statutory retirement age. Our regression model of interest can then simply be written as

$$Death_i^{67} = \beta_0 + \beta_1 ER_i + X_i\beta + \epsilon_i, \quad (2.1)$$

where  $X_i$  denotes additional control variables and  $\epsilon_i$  is the error term. We are interested in estimating parameter  $\beta_1$ , the causal effect of early retirement years (i.e. the number of years between the last day in regular employment and the statutory late retirement age) on premature death (i.e. death before age 67). Since workers self-select into early retirement based on factors that are not observed in the data, e.g. unobserved health shocks,  $ER_i$  is endogenous and thus the simple OLS estimate of  $\beta_1$  is biased.

### 2.4.1 Identification

Our empirical design tackles reverse causality by exploiting the exogenous variation in the date of permanent exit from employment generated by the REBP. As we explained, the REBP allowed eligible workers in treated regions to advance permanent withdrawal from employment by up to 3.5 years. To assess the causal relationship between early retirement and mortality, we use an instrumental variable (IV) approach. Using this empirical strategy, we estimate the causal effect for those individuals whose date of permanent exit from employment is affected by their eligibility to the REBP, i.e. we use workers' REBP eligibility as an instrument for their actual retirement age (e.g. Angrist *et al.*, 1996; Imbens and Angrist, 1994). The credibility of our empirical strategy hinges upon the assumption that our instrument is "as good as randomly assigned". In other words, REBP eligibility should be uncorrelated with unobserved variables that are associated with retirement age and that simultaneously affect the risk of premature death. REBP eligibility was not randomized but a function of age, previous

work experience, and location of residence. Hence REBP eligibility should be considered to be conditionally randomized, where the conditioning is done on the eligibility criteria mentioned above.<sup>17</sup> Since the age and experience criteria are fulfilled by construction of the sample, the question of whether our instrument is valid or not essentially boils down to the question whether the risk of premature death is correlated with individuals' regions of residence in the absence of the program (an issue that we take up in section 2.4.2 below).

An equivalent way of thinking about our empirical design is to consider the eligibility criteria,  $Z_i$  as a deterministic function of a worker's age, work experience, and his or her location of residence. From this perspective, we have to argue that each of these indicator functions is exogenous from an individual's standpoint. Otherwise, it would be possible for an individual to manipulate one (or more) of the variables determining eligibility and thus indirectly manipulate his or her eligibility status. Age and previous work experience are unlikely to be endogenous in the present context.<sup>18</sup> However, endogenous mobility across regions may be an issue since workers may move from non-eligible districts to eligible districts in order to become eligible for the program. While this is a potential problem, it is mitigated by the fact that eligibility rules require residence in a treated region of at least 6 months prior to claiming unemployment benefits. Moreover, mobility is rather uncommon among older workers in Austria. In 1991, for example, only 3 percent (4 percent) of individuals aged 55-59 (50-54) had moved across districts within states or across states within the last 5 years.<sup>19</sup> This suggests that the type of mobility that would cause worries for our empirical strategy is a rather negligible phenomenon.

Another related problem may arise if location of residence has per se an effect on individuals' mortality risk. Location of residence is a REBP eligibility criterion. Conditioning on place of residence at the district level is thus not feasible, since it is perfectly correlated with our instrument. To circumvent this potential problem, we included only those NUTS-3 regions in our sample that comprise both districts eligible to the REBP and those that are not so. If neither mortality risk nor the duration of early retirement is governed by REBP-

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<sup>17</sup>Introducing covariates into the heterogeneous effects model technically calls for the semi-parametric procedure proposed by Abadie (2003). However, no extension of this procedure for models with variable treatment intensity yet exists (i.e. age at retirement is a continuous variable). On the other hand, however, Angrist (2001) argues that 2SLS is likely to give a good approximation to the causal relationship of interest in many cases (i.e. the Abadie procedure is identical to 2SLS when the first stage is linear).

<sup>18</sup>Age can clearly be considered as exogenous in our setting. The employment criteria may be subject to an endogeneity issue if individuals improve their work history in order to become eligible for the program. However, we restrict the sample to individuals with an almost continuous work history (recall from Table 2.1 that the workers in our sample have on average more than 20 employment years during the last 25 years). Since the REBP was only announced shortly before coming into force and was in place for only 5 years, the workers in our sample fulfilled the employment criteria even without altering their work behavior.

<sup>19</sup>The Austrian census asks individuals whether they moved in the past 5 years. According to these data, 88% did not move at all, 5% moved within communities, 1% moved across communities within district, and 2% immigrated from abroad (figures are from census data, Statistics Austria).

eligibility status within any NUTS-3 unit, the independence assumption likely holds, ensuring the validity of our instrument.<sup>20</sup>

The specification of the first-stage regression remains. Based on the previous discussion, we assume that the following equation determines the duration of early retirement

$$ER_i = \alpha_0 + Z_i\alpha_Z + \sum_j C_{ij}\alpha_{Cj} + \sum_k E_{ik}\alpha_{Ek} + \sum_l N_{il}\alpha_{Dl} + X_i\alpha + \varepsilon_i, \quad (2.2)$$

where, as before, the endogenous variable  $ER_i$  corresponds to the number of years spent in early retirement.  $Z_i$  is our binary instrument, denoting whether an individual was eligible (in which case  $Z_i = 1$ ) or not eligible ( $Z_i = 0$ ) to the REBP. The variables  $C_{ij}$ ,  $E_{ik}$ , and  $N_{il}$  refer, respectively, to the workers' date of birth, previous work experience, and NUTS-3 unit of residence, i.e. the three eligibility criteria of the program.<sup>21</sup> We also include additional control variables denoted by  $X_i$  in some specifications.<sup>22</sup> These additional controls increase the precision of our estimates and are helpful in underlining the credibility of our empirical strategy by showing that these additional controls do not have an effect on the 2SLS estimates.

Finally, notice that the REBP was only in effect for a limited period of time. This implies that the various birth cohorts differ in the extent to which the REBP actually offered a pathway to early retirement. For instance, birth cohort 1930 was already 58 years old at the date when the REBP was implemented. In contrast, birth cohort 1933 was 55 years old when the REBP started. The former cohort could take only limited advantage of the program (retiring at age 58), whereas the latter cohort could take full advantage of the program (by already retiring at age 55), as the actual benefits stemming from the program depend on an individual's date of birth. To capture the heterogeneity in the effect of the instrument on the first-stage outcome, we allow for cohort-specific effects by including interaction terms between

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<sup>20</sup>Three additional assumptions are needed, and they are likely to be fulfilled. First, we have to assume that the only channel through which REBP eligibility has an impact on premature death is through its impact on the duration of early retirement. Thus the instrument must not have any direct effect on the dependent variable. We believe that this assumption holds in the present context, as it is difficult to imagine that the mere eligibility to extended benefits should have any direct effect on health and mortality. Second, we assume that the instrument has a monotone impact on the endogenous variable. In our context, we have to assume that REBP eligibility induced *some* individuals to retire earlier than in the absence of eligibility, and that *no* individual decided to retire later because of REBP eligibility. Although we cannot test this assumption, we think it is quite unlikely that this assumption fails in our application. Finally, the REBP eligibility must have an effect on the early retirement date (i.e. the date when individuals permanently leave the labor force). We show in some detail that this is indeed the case in section 2.5.

<sup>21</sup>Specifically,  $j$  indexes half-year-of-birth and runs from 1929h2 to 1941h2 for men and from 1934h2 to 1941h2 for women;  $k$  refers to the past 1, 2, 5, 10, and 25 years (before age 50); and  $l$  indexes those 8 NUTS-3 units included in the analysis. For work experience, we also include squared terms.

<sup>22</sup>The list of additional control variables is as follows: Several terms counting the number of past days on sick leave (also indexed by  $k$ ) and the corresponding squared terms, employers' industry affiliation (14 industries), the log of the average of yearly earnings between ages 43 and 49, and the log of the standard deviation of yearly earnings between ages 43 and 49.

the eligibility indicator and year-semester of birth into the first-stage equation

$$ER_i = \alpha_0 + \sum_j (Z_i \cdot C_{ij}) \alpha_{Zj} + \sum_j C_{ij} \alpha_{Cj} + \sum_k E_{ik} \alpha_{Ek} + \sum_l N_{il} \alpha_{Nl} + X_i \alpha + \varepsilon_i, \quad (2.3)$$

which implies that we now have 25 instruments for our male cohorts (1929h2–1941h2) and 15 instruments for our female cohorts (1934h2–1941h2), respectively.

### 2.4.2 Assessing Instrument Validity

As we have explained, our key identifying assumption is that location of residence in either a treated or a control region is exogenous with respect to individuals' health status. We now provide two pieces of evidence supporting the validity of our instrument.

First, Table 2.2 shows the estimates of a regression of standardized mortality rates at the district level for the years 1978–1984, well before the REBP was implemented. We explore differences in standardized mortality rates at the district level for four different age groups, separately for men (columns (1) to (4)) and women (columns (5) to (8)). The table shows estimates from a simple regression of (district-specific) log standardized mortality rates on a dummy indicating eligible districts. It turns out that standardized mortality rates did not differ between eligible and non-eligible districts before the REBP started. The relevant point estimate turns out to be both statistically and quantitatively insignificant.

The second piece of evidence makes use of individual-level information on workers' days on sick leave provided by the ASSD. This is a good proxy for workers' ex-ante health condition. We measure the number of sick leave days *before* the individual turns age 50, i.e. immediately before he or she meets the age criterion on the REBP. To assess whether eligible and non-eligible individuals have ex-ante similar health conditions, we regress the number of sick leave days on our binary instrument  $Z_i$  while controlling for cohort fixed-effects, experience, NUTS-3 fixed-effects, industry fixed-effects, and earnings. Table 2.3 shows reduced-form results for four different counts of sick leave days, for male and female workers separately. Irrespective of the length of retrospective information used for the sickness indicator, it turns out that workers' health conditions do not systematically differ between eligible and non-eligible individuals within the same NUTS-3 units, and this is valid for both men and for women.

Taken together, we think that the evidence presented in Tables 2.2 and 2.3 provides strong support for our claim that the selection of eligible labor-market districts was unrelated to mortality in these districts.

Table 2.2: Standardized mortality rates, 1978-1984

Age group	Men				Women			
	<45	45-65	65-75	>75	<45	45-65	65-75	>75
Mean	5.0557	7.1355	8.3811	9.5872	4.2331	6.3441	7.7217	9.2952
Standard deviation	0.1491	0.1222	0.0856	0.0630	0.1494	0.1130	0.0975	0.0828
Eligible district	0.0432 (0.0417)	-0.0489 (0.0318)	-0.0045 (0.0220)	0.0008 (0.0139)	-0.0370 (0.0543)	-0.0427 (0.0447)	-0.0078 (0.0233)	0.0069 (0.0215)
Number of Districts	93	93	93	93	93	93	93	93
R <sup>2</sup>	0.0159	0.0303	0.0005	0.0000	0.0116	0.0270	0.0012	0.0013
p-value (F-statistic)	0.3022	0.1272	0.8379	0.9554	0.4968	0.3417	0.7380	0.7502

Notes: \*\*\*, \*\*, \* denotes statistical significance at the 1%, 5%, and 10% level respectively. Robust standard errors in parentheses. The dependent variable is the log of the number of deceases per 100,000 residents. All regressions are weighted by districts' resident population in 1981. Standardized mortality rates account for variation in age distribution across regions. Based on data from Statistics Austria.

Table 2.3: Reduced form effect on sick leave days during the last  $k$  year(s) before age 50

Sick leave days during	Men				Women			
	last year	last 2 years	last 5 years	last 10 years	last year	last 2 years	last 5 years	last 10 years
Mean	5.0261	8.1156	16.9238	39.2707	7.0661	10.6692	19.7779	31.8803
Standard deviation	22.6170	29.7718	45.7849	72.7692	29.0850	35.9460	50.5180	64.2070
Eligible district	-0.2817 (0.3335)	-0.0266 (0.4241)	-0.9548 (0.6492)	-0.8047 (1.0382)	-1.4623 (0.9182)	-1.0209 (1.1431)	0.0717 (1.7162)	-0.6694 (2.2929)
Cohort fixed-effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Experience	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
NUTS-3 fixed-effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Additional controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	17,590	17,590	17,590	17,590	3,283	3,283	3,283	3,283
R <sup>2</sup>	0.2742	0.2721	0.2388	0.2576	0.3655	0.3800	0.3291	0.2719

Notes: \*\*\*, \*\*, \* denotes statistical significance at the 1%, 5%, and 10% level respectively. Robust standard errors in parentheses. There are 25 (15) distinct male (female) cohorts, 10 controls for past work experience before age 50, and 8 distinct NUTS-3 regions. Additional control variables are the log of the average of yearly earnings between ages 43 and 49, the standard deviation of yearly earnings between ages 43 and 49, and employers' industry affiliation (14 industries).



## 2.5 Program Eligibility and Retirement

A first look at descriptive statistics in section 2.3.4 above shows that both males and females withdraw substantially earlier from the work force in eligible regions. We proceed by presenting first-stage estimates of equations (2.2) and (2.3), respectively. Results are given in Table 2.4 for men and Table 2.5 for women, respectively. We will first discuss the results for males.

We show estimates for four different regression specifications. Columns (1) and (2) estimate one common effect of the instrument on the endogenous variable, while columns (3) and (4) allow for a varying effect across birth cohorts. Columns (1) and (3) control for cohort fixed-effects, past work experience, and NUTS-3 fixed-effects; columns (2) and (4) additionally include past sick leave days, the average and standard deviation of yearly earnings (during ages 43 to 49), and industry fixed-effects.

We start with the just-identified case (i.e. estimates of equation (2.2)), shown in the first two columns of each table. For males, the common first-stage effect of the instrument amounts to 0.71 years. This means that REBP-eligibility lowers the effective age of retirement by roughly 8.5 months. If we add further controls in column (2), the effect of the instrument is somewhat reduced to 0.59 years (roughly 7 months). Table 2.5 reports corresponding results for female workers. The first stage effect averaged across birth cohorts amounts to 1.01 years in the first specification and is only slightly reduced to about 0.94 years when additional controls are included (see column (2) of Table 2.5).

Next, we turn to the over-identified case, given by equation (2.3) above. The overall pattern becomes more apparent in a graph. Figure 2.3 displays the relevant parameter estimates,  $\hat{\alpha}_{zj}$ , per year-semester cohort (these estimates correspond to those displayed in column (3) of Table 2.4). The underlying regressions control for cohort fixed effects (one for each year-semester cohort), work experience, and NUTS-3 fixed-effects. Panel (a) shows that the first-stage effect is small for older cohorts and becomes increasingly larger for younger cohorts. This is exactly what we expect, given the REBP rules. Cohorts born in 1929 were already close to 60 years old when the REBP was implemented. Consequently, the REBP cannot have had a sizable impact on the date of permanent exit from the work force for them. The figure shows that the strongest impact is observed for cohorts born in 1934 or later, who could take full advantage of the REBP. This strongly suggests that the REBP entitlement strongly drives the pattern of permanent labor force exit. For female workers, the pattern is similar and the size of the first-stage effect is even more pronounced (see Panel (b)).

Column (3) of Table 2.4 reports the estimates from Panel (a) of Figure 2.3. The first stage effect ranges from 0.031 years (birth cohort 1931h1) to 1.36 years (birth cohort 1937h2). Beginning with birth cohort 1931h2, all estimates are statistically significant at the 1%-level (except for birth cohort 1933h2, which is only marginally significant at the 10%-level). Statistical significance is also reflected in the relevant F statistic, calculated for the excluded



Table 2.4: First-stage results, men

	Retirement years before the statutory retirement age			
	8.6678	8.6678	8.6678	8.6678
Mean	8.6678	8.6678	8.6678	8.6678
Standard deviation	2.9346	2.9346	2.9346	2.9346
Eligible district	0.7100***	0.5895***		
Eligible district · 1929h2			0.3652**	0.0900
Eligible district · 1930h1			0.1658	−0.0254
Eligible district · 1930h2			0.0879	−0.1167
Eligible district · 1931h1			0.0307	−0.0617
Eligible district · 1931h2			0.7390***	0.5466***
Eligible district · 1932h1			0.6021***	0.4285**
Eligible district · 1932h2			0.8066***	0.6584***
Eligible district · 1933h1			0.6284***	0.4503**
Eligible district · 1933h2			0.3868*	0.2533
Eligible district · 1934h1			0.6323***	0.4812**
Eligible district · 1934h2			0.9923***	0.8322***
Eligible district · 1935h1			0.9849***	0.7802***
Eligible district · 1935h2			0.7494***	0.5207**
Eligible district · 1936h1			1.2162***	1.1637***
Eligible district · 1936h2			0.6622***	0.6336***
Eligible district · 1937h1			1.0500***	1.0469***
Eligible district · 1937h2			1.3570***	1.2406***
Eligible district · 1938h1			0.9968***	0.9708***
Eligible district · 1938h2			0.5397**	0.3333
Eligible district · 1939h1			1.1068***	0.9968***
Eligible district · 1939h2			0.7041***	0.5939***
Eligible district · 1940h1			0.8803***	0.9863***
Eligible district · 1940h2			0.9150***	0.8897***
Eligible district · 1941h1			0.9651***	0.9921***
Eligible district · 1941h2			0.6944***	0.5212**
Cohort fixed-effects	Yes	Yes	Yes	Yes
Experience	Yes	Yes	Yes	Yes
NUTS fixed-effects	Yes	Yes	Yes	Yes
Additional controls	No	Yes	No	Yes
Number of observations	17,590	17,590	17,590	17,590
R <sup>2</sup>	0.1326	0.1980	0.1357	0.2021
First Stage F-statistic (Instruments)	243.0828	174.5787	11.9630	10.2984

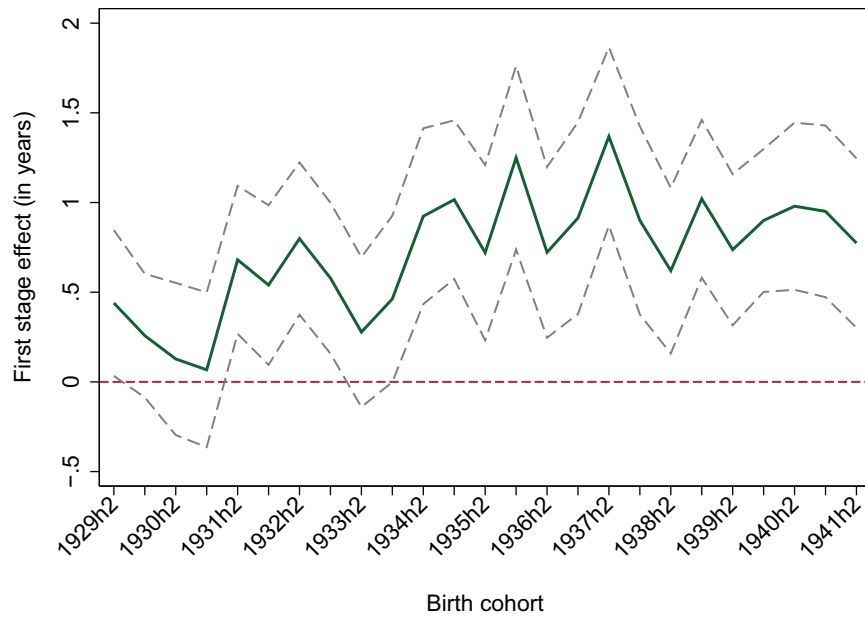
Notes: \*\*\*, \*\*, \* denotes statistical significance at the 1%, 5%, and 10% level respectively. Robust standard errors in parentheses. There are 25 (15) distinct male (female) cohorts, 10 controls for past work experience before age 50, and 8 distinct NUTS-3 regions. Additional control variables are the log of the average of yearly earnings between ages 43 and 49, the standard deviation of yearly earnings between ages 43 and 49, the number of sick-leave days before age 50 (10 terms), and employers' industry affiliation (14 industries).

Table 2.5: First stage effect, women

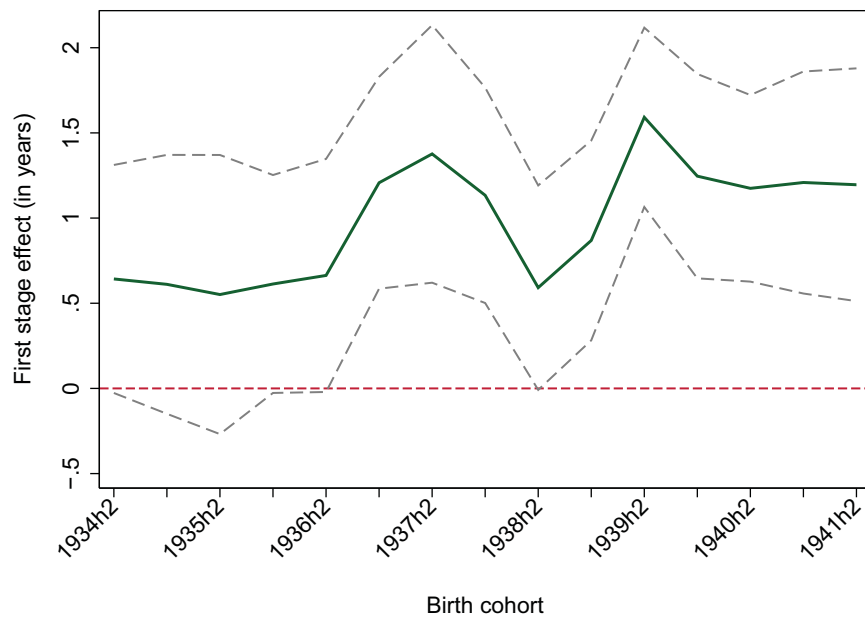
	Retirement years before the statutory retirement age			
Mean	6.4526	6.4526	6.4526	6.4526
Standard deviation	2.2675	2.2675	2.2675	2.2675
Eligible district	1.0104***	0.9399***		
Eligible district · 1934h2			0.6717**	0.5006*
Eligible district · 1935h1			0.3345	0.3267
Eligible district · 1935h2			0.4140	0.2519
Eligible district · 1936h1			0.7349**	0.4873*
Eligible district · 1936h2			0.7092**	0.6789**
Eligible district · 1937h1			1.1734***	0.9597***
Eligible district · 1937h2			1.2091***	1.0138***
Eligible district · 1938h1			1.1244***	0.9275***
Eligible district · 1938h2			0.9883***	1.1578***
Eligible district · 1939h1			0.8349***	0.8560***
Eligible district · 1939h2			1.6288***	1.6173***
Eligible district · 1940h1			1.1276***	1.1186***
Eligible district · 1940h2			1.3271***	1.2925***
Eligible district · 1941h1			1.2916***	1.3071***
Eligible district · 1941h2			1.1064***	1.0060***
Cohort fixed-effects	Yes	Yes	Yes	Yes
Experience	Yes	Yes	Yes	Yes
NUTS fixed-effects	Yes	Yes	Yes	Yes
Additional controls	No	Yes	No	Yes
Number of observations	3,283	3,283	3,283	3,283
R <sup>2</sup>	0.1721	0.2489	0.1779	0.2558
First Stage F-statistic (Instruments)	153.1078	137.4533	11.9306	11.2351

Notes: \*\*\*, \*\*, \* denotes significance at the 1%, 5%, and 10% level respectively. Robust standard errors in parentheses. There are 25 (15) distinct male (female) cohorts, 10 controls for past work experience before age 50, and 8 distinct NUTS-3 regions. Additional control variables are the log of the average of yearly earnings between ages 43 and 49, the standard deviation of yearly earnings between ages 43 and 49, the number of sick-leave days before age 50 (10 terms), and employers' industry affiliation (14 industries).

Figure 2.3: First-stage results



(a) Men



(b) Women

Notes: The figures plot the difference in the retirement age between eligible and non-eligible districts by year-semester birth-cohort in the sample of male and female workers, respectively. Dashed lines show 95% confidence bands.

instruments only and reported at the bottom of the table. It amounts to 12, i.e. it is larger than the threshold value of 10 above which 2SLS is not supposed to be subject to a weak instruments critique as proposed by Staiger and Stock (1997). Adding further controls again reduces the magnitude of the first-stage effect somewhat, but the F statistic for the excluded instruments is still slightly larger than 10.<sup>23</sup>

Column (3) of Table 2.5 shows the corresponding point estimates for women, displayed graphically in Panel (b) of Figure 2.3. The first-stage effect varies across birth cohorts, ranging from about 0.33 years (birth cohort 1935h1) to about 1.63 years (birth cohort 1939h2). Starting with birth cohort 1936h1, all coefficients are statistically significant at the 1%-level. Adding further controls in column (4) hardly changes anything. The F statistic for the excluded instruments exceeds the value of 10 in both column (3) and column (4). This again suggests that we do not run into any weak-instruments issues.

### Treatment Intensity

Figure 2.4 takes a closer look at the distribution of the effective age at retirement by eligibility status, for men and women separately. More precisely, the figure shows the difference in the survivor function of still being in employment at a given age between individuals from eligible versus non-eligible regions. The difference measured on the vertical axis of the figure is negative throughout, indicating that the fraction of workers still at work at any particular age is lower in eligible regions than in non-eligible regions.

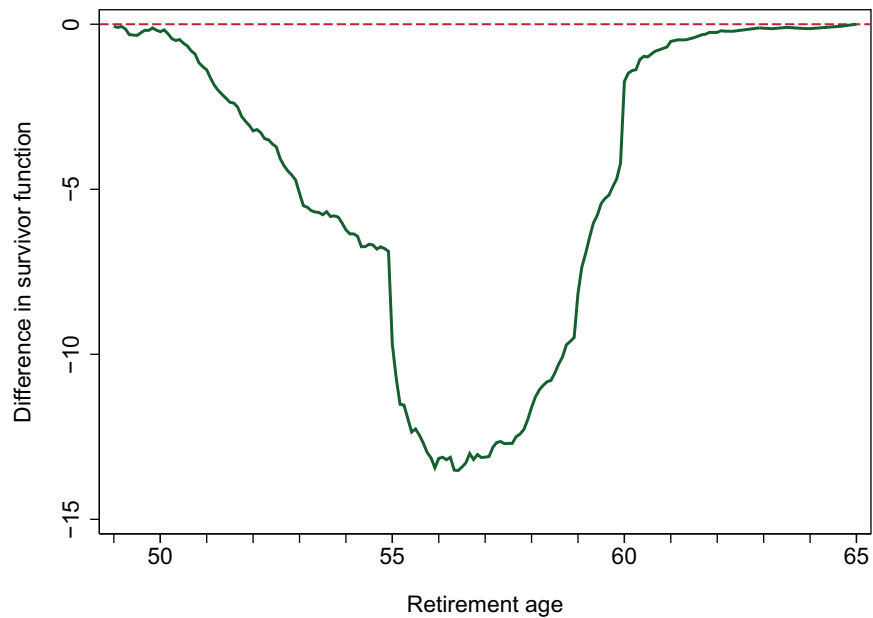
We showed above that the individuals retiring between age 55 and 59 are those who drive these effects. This exactly is what we expect from the institutional rules: workers eligible to extended unemployment benefits due to the REBP can already retire at age 55, draw regular unemployment benefits until the age of 59, and then draw benefits from special income support before they become eligible to regular early retirement benefits at the age of 60. Workers in non-eligible regions have no access to extended unemployment benefits and can first claim special income support at age 59. Male blue collar workers eligible to the REBP are 9-14% less likely to be in employment within the age bracket 55-59. As a consequence, our IV estimates capture the causal effect of changes in the retirement age within this age bracket, but tell us little, if anything, about the effects of retiring between the statutory retirement age with long insurance duration (60/55) and the statutory retirement age (65/60).<sup>24</sup>

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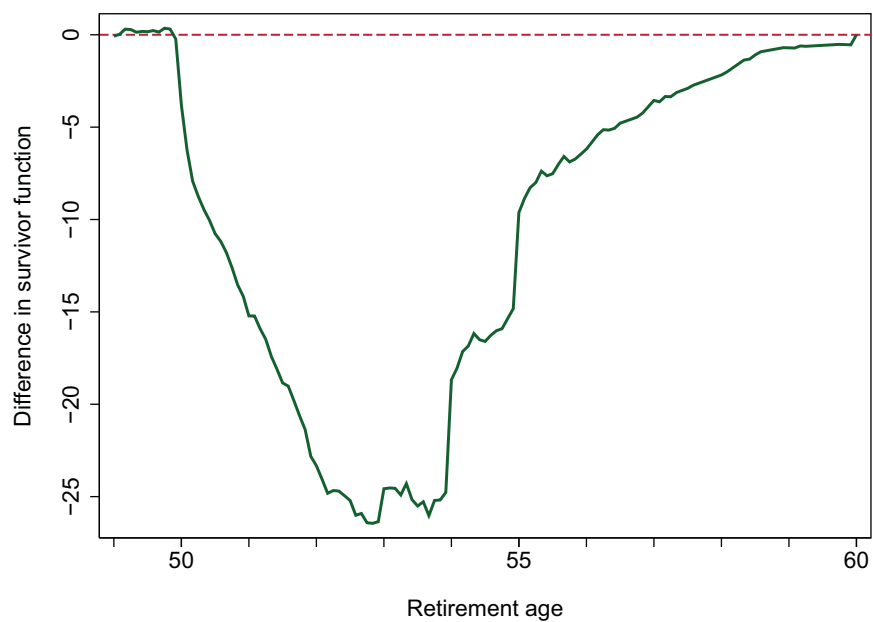
<sup>23</sup>Table A.1 in the appendix provides evidence on whether the REBP really causes the contrast in the retirement age, or whether this is simply due to regional differences between eligible and non-eligible districts. It shows the first-stage for cohorts who are not eligible to the REBP (i.e. workers aged less than 50 when the REBP ends). It turns out that no systematic difference emerges between eligible and non-eligible districts for cohorts too young for extended UB entitlement. This strengthens our claim that the contrast in the effective retirement age is causally linked to the REBP.

<sup>24</sup>Early retirement also involves a substitution among different labor market activities. Figure A.1 in the appendix shows how eligible and non-eligible workers differ with respect to labor market activities. The

Figure 2.4: Treatment intensity



(a) Men



(b) Women

Notes: The figures show the difference in the survivor function (i.e. the probability of still being employed at a given age) between individuals from eligible and non-eligible regions.

## 2.6 Early Retirement and Mortality

Tables 2.6 and 2.7 report our main results for blue collar males and females, respectively. Column (1) of Table 2.6 shows the OLS estimates of a regression of the number of early retirement years on mortality for blue-collar males. The regression controls for birth-cohort fixed-effects, work experience, and NUTS-3 fixed-effects. The OLS estimate is highly significant and amounts to 0.0322 (with a standard error of about 0.0011). Taken literally, this would imply that the probability of dying before age 67 increases by 3.22 percentage points for each year of early retirement. In terms of the average probability of dying before age 67 (equal to about 18.0%), this corresponds to a relative increase of about 17.9%. The inclusion of additional controls does not change the OLS estimate. However, as argued before, OLS estimates are likely plagued by endogeneity bias due to non-random selection into early retirement.

Columns (3) to (6) show our 2SLS results. In the just-identified case (i.e. columns (3) and (4)), we get a much smaller point estimate than the corresponding OLS estimate. Using the minimal (extended) set of control variables yields an IV estimate of 0.0078 (0.0122) compared to the corresponding OLS estimate of 0.0322 (0.0324). Moreover, the IV estimate turns out to be statistically insignificant in both cases. In the over-identified case shown in column (5), we get a point estimate of about 0.016 (standard error of 0.0078), a decrease in magnitude of about 50% compared to the corresponding OLS estimate. Even though the standard error of this estimate is much larger than that in the corresponding OLS regression, the effect remains statistically different from zero at the 5%-level. Adding further controls in column (6) leads to an even larger point estimate of 0.0242. This estimate is slightly larger than that from column (3), but it is still about a quarter smaller than the OLS estimate. The estimated standard error is 0.0086, resulting in statistical significance at the 1%-level. Based on the 2SLS estimate in column (5) and (6), respectively, one additional year spent in early retirement increases the risk of dying before age 67 by 0.0162 (0.0242) percentage points. Evaluated at the sample mean of the dependent variable (equal to 0.18), this means a relative increase in the risk of premature death of about 9% (13.4%). Moreover, the comparison between OLS and 2SLS estimates clearly shows that the OLS estimates are contaminated by reverse causality and

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left-hand panel shows that workers eligible to the REBP spend less time in employment at ages 50-65 than non-eligible workers. If eligibility to extended unemployment benefits drives earlier effective retirement of blue collar workers in eligible regions, we should see more workers on unemployment benefits after permanent exit from employment. This is exactly what we find: eligible workers spend more than 2 percentage points more of their time on unemployment benefits than non-eligible workers. Apparently, the instrument induces individuals to retire earlier by means of the extended unemployment as a channel from work to retirement by first claiming extended unemployment benefits before accessing regular retirement benefits. The right-hand panel shows that eligible workers substitute regular old-age pension with unemployment benefits after they permanently drop out of employment. The figure also shows that time spent out of the labor force does not substantially differ across the two groups (at least for men). In sum, this strongly suggests a pattern of labor market behavior that is consistent with the incentives generated by the REBP.

Table 2.6: Second stage results, men

	Death before age 67											
	OLS				2SLS				LIML			
	0.1803	0.1803	0.1803	0.1803	0.1803	0.1803	0.1803	0.1803	0.1803	0.1803	0.1803	0.1803
Mean	0.3845	0.3845	0.3845	0.3845	0.3845	0.3845	0.3845	0.3845	0.3845	0.3845	0.3845	0.3845
Standard deviation												
Retirement years before age 65	0.0322*** (0.0011)	0.0324*** (0.0011)	0.0078 (0.0088)	0.0122 (0.0106)	0.0162** (0.0078)	0.0242*** (0.0086)	0.0144* (0.0086)	0.0231** (0.0096)				
Log(average yearly earnings (age 43-49))	-0.1001*** (0.0138)	-0.1003*** (0.0140)				-0.1001*** (0.0138)		-0.1001*** (0.0138)				
Log(std. dev. of yearly earnings (age 43-49))	0.0172*** (0.0047)	0.0174*** (0.0047)				0.0173*** (0.0047)		0.0173*** (0.0047)				
Cohort fixed-effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Experience	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
NUTS fixed-effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Additional controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Instrument interacted with year-semester of birth	-	-	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	17,590	17,590	17,590	17,590	17,590	17,590	17,590	17,590	17,590	17,590	17,590	17,590
R <sup>2</sup>	0.0686	0.0744	0.0380	0.0551	0.0553	0.0712	0.0522	0.0703				

Notes: \*\*\*, \*\*, \* denotes statistical significance at the 1%, 5%, and 10% level respectively. Robust standard errors in parentheses. There are 25 (15) distinct male (female) cohorts, 10 controls for past work experience before age 50, and 8 distinct NUTS-3 regions. Additional control variables are the number of sick-leave days before age 50 (10 terms), and employers' industry affiliation (14 industries).

tend to be too big, which implies that there is selection into early retirement based on ill health. We chose column (6) of Table 2.6 as our preferred estimate and refer to it as such in the following.

Furthermore, as proposed by Angrist and Pischke (2009), we compare the 2SLS estimates with those produced by the limited information maximum likelihood (LIML) estimator in the over-identified case.<sup>25</sup> Column (7) corresponds to column (5) except for the fact that the parameters are estimated by LIML rather than 2SLS. LIML estimation yields a point estimate of 0.0144 (standard error of 0.0086). Analogously, column (8) is the LIML estimate that corresponds to the 2SLS estimate shown in column (6). Here we get an estimate of 0.0231 (standard error of 0.0096). In both cases, the LIML estimates are very similar to the 2SLS estimates (though, as expected, less precise than 2SLS). However, both are still statistically significant at least at the 10%-level. Overall, the comparison between 2SLS and LIML estimates does not suggest that finite-sample bias is a problem (this is not a surprise taking into account that this estimate is based on 17,590 observations).

Our IV-estimates suggest that early exit from the labor force strongly increases mortality.<sup>26</sup> Our preferred estimate of 0.0242 implies that one additional year of early retirement increases the probability of dying before age 67 by as much as 2.4 percentage points. Evaluated at the average probability of dying before the age of 67 (which is equal to 18.0 percent), this corresponds to a relative increase of about 13.4%.

Table 2.7 shows the corresponding results for female blue-collar workers. The first two columns again report OLS results first. Female workers have a probability of dying before the age of 67 that is increased by about 0.81–0.85 percentage points for each year spent in early retirement. The magnitude of this conditional correlation is roughly a third smaller than the corresponding effect found for their male counterparts, but this is still a non-negligible correlation (in relative terms this is an effect of 11.8%, a magnitude comparable to their male counterparts). However, and in contrast to our results for men, this effect vanishes completely once we apply the 2SLS estimation (see columns (3) and (5)). The 2SLS estimates tell us that female workers' earlier exit from the work force has no impact on mortality. Again, the corresponding LIML estimates do not indicate that the 2SLS estimates in columns (5) and (6) suffer from small sample bias since LIML yields estimates very close in magnitude to 2SLS coefficients.

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<sup>25</sup>The more instruments there are, the more relevant issues with weak instruments eventually become. LIML is less biased than 2SLS in finite samples with many instruments, but also has a higher variance.

<sup>26</sup>One might argue that our estimates are driven by individuals dying while still working, a situation that is in principle possible. Indeed, this may bias our results if death at work occurs with different probability in eligible versus non-eligible districts. To investigate this issue in more detail, we constructed a subsample in which all workers are excluded who die within three months after leaving employment (about 270 male individuals) and then re-estimated our main models. The results remain quantitatively very similar to those presented in Table 2.6 (results are available upon request).

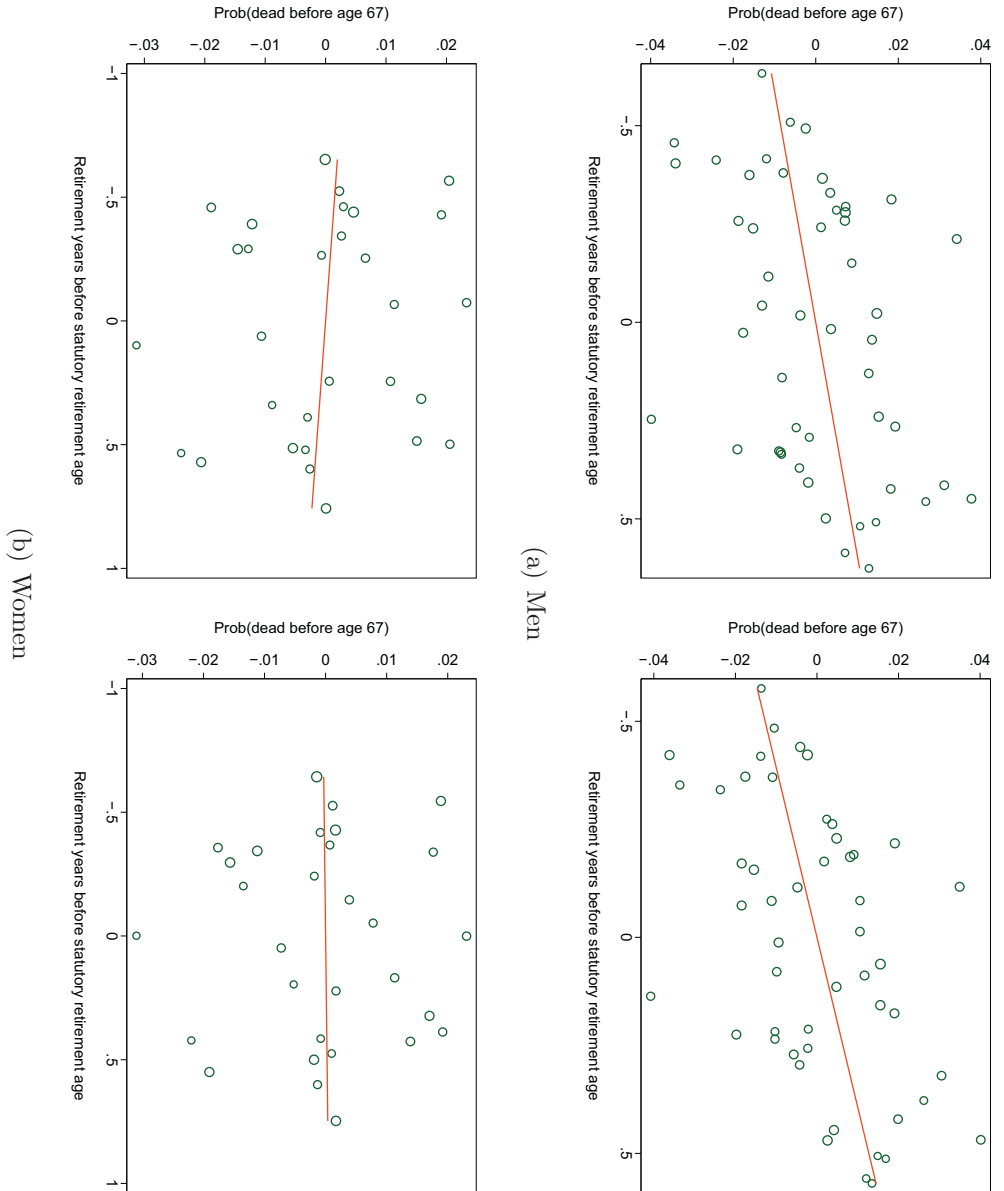


Table 2.7: Second stage results, women

	Death before age 67									
	OLS			2SLS			LIML			
Mean	0.0719	0.0719	0.0719	0.0719	0.0719	0.0719	0.0719	0.0719	0.0719	
Standard deviation	0.2583	0.2583	0.2583	0.2583	0.2583	0.2583	0.2583	0.2583	0.2583	
Retirement years before age 60	0.0081*** (0.0021)	0.0085*** (0.0023)	-0.0051 (0.0096)	-0.0016 (0.0104)	-0.0032 (0.0089)	0.0002 (0.0095)	-0.0038 (0.0094)	-0.0003 (0.0099)		
Log(average yearly earnings (age 43-49))		0.0152 (0.0146)		0.0212 (0.0162)		0.0202 (0.0159)		0.0205 (0.0160)		
Log(std. dev. of yearly earnings (age 43-49))		-0.0016 (0.0062)		-0.0026 (0.0062)		-0.0024 (0.0062)		-0.0025 (0.0062)		
Cohort fixed-effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	
Experience	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	
NUTS fixed-effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	
Additional controls	No	Yes	No	Yes	No	Yes	No	Yes	Yes	
Instrument interacted with year-semester of birth	-	-	No	No	Yes	Yes	Yes	Yes	Yes	
Number of observations	3,283	3,283	3,283	3,283	3,283	3,283	3,283	3,283	3,283	
R <sup>2</sup>	0.0184	0.0280	0.0069	0.0219	0.0099	0.0238	0.0090	0.0234	0.0234	

Notes: \*\*\*, \*\*, \* denotes statistical significance at the 1%, 5%, and 10% level respectively. Robust standard errors in parentheses. There are 25 (15) distinct male (female) cohorts, 10 controls for past work experience before age 50, and 8 distinct NUTS-3 regions. Additional control variables are the number of sick-leave days before age 50 (10 terms), and employers' industry affiliation (14 industries).

Figure 2.5: Visual representation of IV-estimates



Notes: The figures plot the probability of premature death (before age 67) against the number of retirement years. More specifically, the figures plot average residuals from a regression of the probability of death before age 67 (vertical axis) and the number of retirement years before the statutory retirement age (horizontal axis) on cohort fixed effects, NUTS-3 fixed effects and controls for past work experience (left-hand figures). The figures on the right-hand side plot residuals from regressions that additionally include industry fixed effects as well as controls for sick-leave days and earnings.

Our IV strategy in the over-identified case lends itself to a simple graphical representation, which is given by Figure 2.5. The visualization builds on the equivalence of 2SLS using a set of dummy instruments and GLS on grouped data, where the grouping is done over the dummy instruments (this equivalence is elaborated in Angrist, 1991). Briefly, the left-hand panel of Figure 2.5 shows the relationship between the probability of being eligible to the REBP on the horizontal axis and the probability of dying before age 67 on the vertical axis (which in turn may be understood as a plot of the reduced form against the first-stage). The figure plots average residuals by year-semester date of birth and eligibility status from a regression of the dependent variable (the endogenous variable, respectively) on cohort fixed-effects, NUTS-3 fixed effects, and controls for past work experience (using corresponding cell sizes as weights). The right-hand panel of Figure 2.5 shows average residuals from regressions that include additional control variables (corresponding to regression specification shown in column (6) in Table 2.6). The figure clearly shows that there is a positive causal relation between the number of early retirement years and the probability of premature death (before age 67) for male workers. In contrast, Panel (b) of Figure 2.5 shows that no such relation exists for female workers.

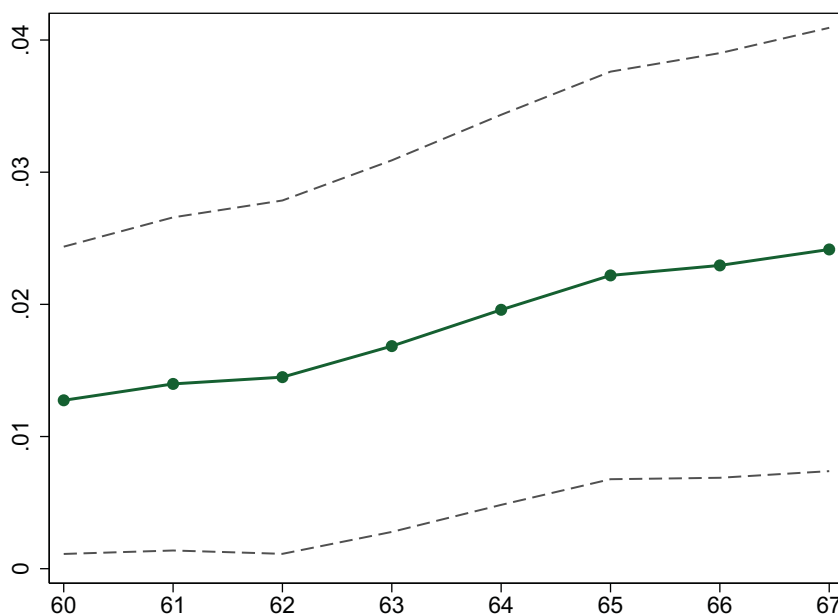
### Years of Life Lost

While our dependent variable, death before age 67, is precisely defined, it does not tell us whether and to which extent early retirement affects life expectancy. Calculating the impact on life expectancy is not straightforward because the underlying mortality hazard is nonlinear in age and because we observe actual mortality only until age 67 for all individuals in our sample. To convert our estimated effect of into years of life lost, we need to impose further assumptions. To get a benchmark for the impact of early retirement on life expectancy, we assume that differences in survival rates between treated and controls occur between age 60 and age 67 only and that there are no retirement-effects on mortality rates (i.e. non-survivor rates) outside this age bracket. Under this assumption, the cumulative difference in survivor rates between treated and non-treated workers in the age bracket 60 to 67 yields an estimate for the impact on life expectancy.<sup>27</sup> If early retirement affects mortality also outside this age range, our estimated impact of early retirement on life expectancy will be biased (where the bias may go in both directions). As almost all individuals in our sample retire before age 60 (only 1.4% retire after age 60), we can provide meaningful estimates for each premature death indicator defined as the occurrence of death before age 60,...,67 in the same way as we did in our main analysis for death before age 67. Figure 2.6 shows estimates for premature death

<sup>27</sup>Denoting by  $T$  the duration of life after age 50, expected remaining life expectancy at age 50 is given by  $E(T) = \sum_{t=51}^{\infty} S(t)$  (Lancaster, 1992, p.13). Assuming that differences in mortality arise only within ages 60 and 67, the change in remaining life expectancy is given by  $\Delta E(T) = \sum_{t=61}^{67} \Delta S(t) = -\sum_{t=61}^{67} \Delta F(t)$  where  $F(t)$  is equal to  $1 - S(t)$ . Note that  $F(t)$  is the dependent variable in all our regressions.

before age 60,...,67. It turns out that the probability of death before age 60 is significantly higher among eligible than non-eligible workers. The estimated effect increases with age and about doubles in absolute size by age 67 (where the rightmost point estimate in Figure 2.6 is the main estimate from column (6) of Table 2.6).

Figure 2.6: 2SLS estimates of early retirement on premature death



Notes: The figure shows 2SLS estimates (and corresponding 95% confidence intervals) of early retirement on premature death before age 60,...,67 (using the same model specification as in column (6) of Table 2.6).

To calculate the difference in life expectancy that arises due to differences in survivor rates in the age bracket 60 to 67, we simply add up the eight estimated differences in survivor rates shown in figure 2.6 which yields 0.15 years. More precisely, our estimates indicate that one additional year of early retirement reduces life expectancy of male blue collar workers by 0.15 years or about 1.8 months. Recall that this estimate is valid only if all differences in survivor rates occur between ages 60 and 67 – and that the (cumulative) difference in survivor rates between treated and control groups outside the age bracket 60 to 67 is zero. This estimate is biased upward if the cumulative difference outside this age bracket is higher among treatment groups and vice versa.

## 2.7 Potential Channels

We now explore several potentially important channels that might help explain the observed increased mortality among male blue collar early retirees. We first show that losses in earnings

associated with early retirement are quite small and thus cannot be the main explanation of the evident excess mortality among male workers. Second, we use ancillary information to investigate whether the detrimental impact of early retirement on mortality can be ascribed to specific death causes. Third, we provide some suggestive evidence on the impact of retirement voluntariness on the estimated effect of early retirement on premature death. As the preceding section has shown no causal effect of early retirement on premature death for women, the analysis in this section is confined to male blue collar workers only.

### 2.7.1 Loss of Earnings

Earnings losses may contribute to an explanation of excess mortality among early retirees. To check the relevance of this channel, we first estimate the reduction in permanent earnings for individuals aged 50 or older if they retire one year earlier. We find that the reduction in permanent income for individuals aged 50 or older is only about 2.5 percent.<sup>28</sup> Taken at face value, the estimated OLS estimate of -0.10 for the effect of average earnings before the age of 50 on mortality would imply that we expect an increase in the probability of dying before age 67 of about 0.25 percentage points.<sup>29</sup> We therefore conclude that at most 10% of our preferred estimate of the causal effect of retirement on premature mortality can be explained by the reduction in permanent income associated with early retirement.<sup>30</sup>

The income channel in our case is much less important than that in a recent study by Sullivan and Wachter (2009), who find that this specific channel accounts for as much as 50%–75% of the overall effect of involuntary job loss on mortality in the US. The fact that there is compulsory and universal health insurance coverage in Austria reconciles this difference, however. Moreover, the reduction in income after retiring early is mitigated by relatively high income replacement rates in the Austrian pension system. In sum, we conclude that earnings losses associated with early retirement are too small to provide a credible explanation for our finding of excess mortality among males.

### 2.7.2 Changes in Health-Related Behavior

This section investigates whether changes in individuals' health-related behavior (such as excessive drinking and/or smoking, an unhealthy diet, and a low level of physical activity) can

<sup>28</sup>See Table A.2 in the appendix. Note further that the volatility of income is a minor issue only in our context because income streams are constant as soon as an individual draws pension benefits.

<sup>29</sup>The OLS estimate is taken from column (2) of Table 2.6. Based on this estimate, a reduction in permanent income of 2.5% implies an increase in the probability of death before age 67 of approximately  $-(-0.010/100) \cdot 0.025 = 0.0025$ . This figure is likely to overestimate the effect of earnings on mortality because the OLS estimate of the effect of earnings on premature death is arguably biased upward.

<sup>30</sup>This number results from dividing the estimated effect of the reduction in permanent income of 0.0025 by our preferred 2SLS estimate of 0.0242, taken from column (6) of Table 2.6.

explain the increased risk of premature death among male blue collar workers. In fact, there is considerable – though not conclusive – medical research on the relation between retirement and smoking (e.g. Ekerdt *et al.*, 1989; Lang *et al.*, 2007; Midanik *et al.*, 1995), retirement and (excessive) alcohol use (e.g. Neve *et al.*, 2000; Perreira and Sloan, 2002), as well as between retirement and changes in diet and physical activity (e.g. Chung *et al.*, 2009a,b; Evenson *et al.*, 2002; Mein *et al.*, 2005; Slingerland *et al.*, 2007).

We shed light on this channel by investigating whether early retirement increases the risk of specific causes of death that are directly or indirectly attributable to changes in health related behavior.<sup>31</sup> For this analysis we additionally rely on individual data on mortality provided by Statistics Austria which contains the universe of death cases in Austria. It contains information about the detailed causes of death according to the 9th and 10th revision of the International Classification of Diseases and Related Health Problems (ICD-9, ICD-10). While information on causes of death from Statistics Austria cannot be linked directly with the ASSD (there is no common person identifier), it is nonetheless possible to exactly match information on the basis of four characteristics that are available in both data sets: year and month of birth, year and month of death, NUTS-3 unit, and eligible/non-eligible district. It turns out that cause of death can be unambiguously matched for 2,454 observations (among those 3,172 blue collar workers in our sample who died before age 67) which implies a matching rate of 77.4%. For 147 observations the matching is ambiguous and for 571 observations in the ASSD there is no corresponding observation in the data from Statistics Austria.

In the following we concentrate on the following causes of death: (i) Alcohol-related causes, (ii) ischemic heart diseases, (iii) smoking-related causes (other than ischemic heart disease), (iv) vehicle accidents, and (v) other causes. The assignment of particular diseases to “alcohol-related causes” and “smoking-related causes” is based on the procedure applied by the U.S. Department of Health and Human Services (Table A.3 in the appendix details this classification procedure). We assign deaths to alcohol-related and smoking-related causes if at least 40% of deaths in an ICD category are attributable to excessive consumption of alcohol or smoking, respectively. “Ischemic heart diseases” (mostly heart attacks) are also highly attributable to smoking, and, in addition, to overweight and obesity which are related to an unhealthy diet and a low level of physical activity.<sup>32</sup> “Vehicle accidents” are also to a non-negligible extent

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<sup>31</sup>This is similar to Bedard and Deschênes (2006) who use cause-specific mortality rates to investigate excess mortality among World War II and Korean War Veterans in the U.S. They find that military-induced smoking drives most of the observed excess mortality.

<sup>32</sup>Ischemic heart diseases are indicated by ICD-9 codes 410-414, 429.2 and ICD-10 codes I20-I25. According to the Smoking-Attributable Mortality, Morbidity, and Economic Costs (SAMMEC) application provided by the Centers for Disease Control and Prevention (CDC), one of the major operating components of the U.S. Department of Health and Human Services, the proportion of deaths due to ischemic heart diseases for U.S. males aged 35–64 (65 and above) in the year 2001 attributable to smoking amounts to 40% (15%). For obesity see the study by the U.S. Department of Health and Human Services (2001). There is a broad consensus in the medical literature that there are only a few main risk factors associated with cardiovascular infarction and

attributable to alcohol abuse.<sup>33</sup> “Other causes” are the residual category which contains all remaining death causes as well as those deaths for which the cause of death is unknown due to the failure to link the causes of death statistics with the ASSD.

The results for the cause-specific mortality are displayed in Table 2.8. Because the results without and with the inclusion of additional controls are very similar, the table only reports the results with additional controls. Column (1) repeats the estimate of column (6) of Table 2.6 that shows that premature death (before age 67) increases by 2.4 percentage points for each additional year spent in early retirement. The causes of death displayed in the table are exhaustive and mutually exclusive, thus the estimates from columns (2) to (6) add up to the overall estimate from column (1) (and the mortality rates for the particular death causes sum up to the total mortality rate). Column (2) shows that one year spent in early retirement increases the probability of dying from alcohol-related diseases by 0.71 percentage points. In other words, the risk of dying from diseases (partially) caused by excessive alcohol consumption contributes 29% ( $=0.0071/0.0242$ ) to the overall effect. Column (3) shows that the risk of dying before age 67 due to ischemic heart diseases is increased by 0.94 percentage points, and therefore ischemic heart diseases account for 39% ( $=0.0094/0.0242$ ) of the total effect. The contribution of vehicle injuries (column (5)) amounts to another 0.24 percentage points (or 10% in terms of the total effect). Interestingly, the risk of dying from smoking-related diseases (other than ischemic heart diseases) does not significantly increase due to early retirement (column (4)). The risk of dying from other causes is not significantly affected by early retirement (column (6)).<sup>34</sup> Taken together, alcohol-related causes, ischemic heart diseases, and vehicle injuries account for as much as 78% of the overall causal effect of early retirement. This implies a strong concentration of excess mortality among blue collar males to three causes (which account for 24% of all deaths in our male sample).

Clearly, not all those deaths can directly be attributed to underlying changes in health-related behavior. For instance, only 40% of all deaths caused by portal hypertension can directly be attributed to alcohol abuse (Table A.3 shows the respective attributable fractions). To account more directly for excessive alcohol consumption and smoking as causes of excess

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coronary heart disease. Among the most important risk factors are smoking, hypertension, diabetes, obesity, and psychosocial factors, while a healthy diet (e.g. eating fruit and vegetables) and regular physical exercise appear to be protective (Canto and Iskandrian, 2003; Greenland *et al.*, 2003; Yusuf *et al.*, 2004)

<sup>33</sup>Vehicle accidents are indicated by ICD-9 codes 800-848 and ICD-10 codes V00-V99. According to the U.S. National Highway Traffic Safety Administration (2002) 28% (13%) of all motor-vehicle accidents of U.S. males aged 55-64 (65 and older) are related to alcohol.

<sup>34</sup>We also investigate several subsets of the remaining causes in Table A.4. This Table shows analogous results for the following subcategories: Alcohol-unrelated digestive system diseases, non-ischemic heart diseases, smoking-unrelated respiratory diseases, smoking-unrelated cancer, self-inflicted injuries, other injuries, cerebrovascular diseases, and all remaining causes. It turns out that none of those cause specific deaths are affected by early retirement and thus do not contribute to the overall impact of early retirement on premature death. This strongly supports the notion that the alcohol-related causes, ischemic heart diseases, and vehicle injuries are the driving force for the detrimental impact of early retirement on premature death.



Table 2.8: Causes of death, men only

	Death before age 67	Alcohol- related causes	Ischemic heart disease	Other smoking- related causes	Vehicle injury	Other causes
Mean	0.1803	0.0138	0.0271	0.0271	0.0023	0.1101
Standard deviation	0.3845	0.1165	0.1623	0.1623	0.0482	0.3130
Retirement years before age 65	0.0242*** (0.0086)	0.0071*** (0.0027)	0.0094** (0.0037)	-0.0027 (0.0037)	0.0024** (0.0011)	0.0079 (0.0072)
Cohort fixed-effects	Yes	Yes	Yes	Yes	Yes	Yes
Experience	Yes	Yes	Yes	Yes	Yes	Yes
NUTS fixed-effects	Yes	Yes	Yes	Yes	Yes	Yes
Additional controls	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	17,590	17,590	17,590	17,590	17,590	17,590
R <sup>2</sup>	0.0712	0.0118	0.0097	.	.	0.0292

Notes: \*\*\*, \*\*, \* denotes statistical significance at the 1%, 5%, and 10% level respectively. Robust standard errors in parentheses. A missing  $R^2$  denotes a negative R-squared. The causes of death are classified by means of ICD-9 and ICD-10. See Table A.3 for the ICD codes of *alcohol- and smoking-related causes*. *Ischemic heart diseases* include the the ICD codes 410-414, 429.2 (ICD-9) and I20-I25 (ICD-10). *Vehicle injuries* include the ICD codes 800-848 (ICD-9) and V00-V99 (ICD-10). *Other causes* include all remaining causes as well as those causes that are unknown due to failure of the match between data on causes of death and the ASD.

mortality, we multiply the estimated contribution of each cause to the overall effect by their respective fraction of these deaths that are attributable to alcohol consumption and smoking behavior. The fractions we use for this calculation are as follows: 58% of diseases classified as “alcohol-related causes” are directly attributable to excessive alcohol consumption;<sup>35</sup> 34% of ischemic heart diseases are caused by smoking;<sup>36</sup> and roughly 26% of vehicle injuries are caused by alcohol consumption. This suggests that the contribution of smoking and excessive alcohol consumption amounts to as much as 32.4% ( $= (0.58 \cdot 0.0071 + 0.34 \cdot 0.0091 + 0.26 \cdot 0.0024) / 0.0242$ ) of total excess mortality. Clearly, unhealthy practices are not only confined to smoking and drinking but also to other dimensions such as unhealthy diet and lack of physical activity. Unhealthy diet and lack of physical activity result in overweight and obesity which are themselves important underlying reasons for ischemic heart diseases (see U.S. Department of Health and Human Services (2001)). Hence the contribution of unhealthy behaviors to excess mortality among blue collar males is likely to be much higher than the 32.4% we derived from smoking- and drinking-attributable causes only. We conclude that detrimental changes in health-related behaviors are a major reason for excess mortality among blue collar early retirees.

### 2.7.3 Voluntary or Involuntary Retirement?

Another hypothesis is related to firing decisions of firms. Since the REBP mitigated economic hardships associated with unemployment of older workers, the implementation of this program made it easier for firms to release older workers. If these firm decisions underlie the estimated treatment effects, we should see a larger effect among released workers as opposed those who voluntarily quit their jobs (Henkens *et al.*, 2008; van Solinge and Henkens, 2007).<sup>37</sup>

While it is not possible to directly distinguish between quits and layoffs in our data, we can exploit the institutional particularity that there are sharp discontinuities in eligibility for severance pay in Austria. After 3 years of continuous work history with the same employer, a worker becomes eligible for severance payments. Severance payments amount to twice the monthly salary and increase to three salaries after 5 years, to four after 10 years, to six after

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<sup>35</sup>This corresponds to the weighted average of the attributable fractions (the weights are the share of individuals dying of the specific alcohol-related diseases listed in Table A.3).

<sup>36</sup>34% corresponds to the weighted average of the age dependent smoking attributable fractions regarding ischemic heart diseases (see footnote 32; the weights are the share of individuals dying of ischemic heart diseases before and after age 65).

<sup>37</sup>Of course, there are other potential sources of treatment effect heterogeneity. One especially interesting dimension is workers’ ex-ante health status because it is easily imaginable that mortality effects be predominantly driven by workers with weak ex-ante health. Appendix table A.5 sheds light on this issue. The mortality effect is strong and highly significant ex-ante among workers who are healthier. This suggests that effective early retirement causes premature death by adding to already existing health problems. In contrast, we see that the mortality effect is small and insignificant among workers who are ex-ante healthier.

15 years, to nine after 20 years, and to twelve monthly salaries after 25 years of continuous work history with the same employer. Given that the financial stakes involved are quite high, one might argue that a comparison of workers just above and below any given threshold may be informative about the degree of retirement voluntariness. More specifically, it may be reasonable to assume that the probability of a voluntary quit is higher, *ceteris paribus*, if a worker has just crossed any of the tenure thresholds above, and thus received severance pay, compared to the situation that he just failed to cross the threshold (and thus had to forego [increased] severance pay). Before the threshold around 10, for example, the worker only gets three months of severance pay and might be sorely tempted to wait around to get six. If he goes before ten years, he does not lose severance pay, but receives a reduced amount.

Table 2.9 shows the resulting estimates using two different subsamples. The first (second) subsample contains only male workers with job tenure in a range of up to 6 (12) months around any tenure threshold relevant for severance pay (i.e. 3, 5, 10, 15, 20, or 25 years of job tenure). We then re-estimate, for each of the two subsamples, our main models of columns (5) and (6) of Table 2.6 for those workers below or above any existing tenure threshold relevant for severance pay. The first four columns show estimates based on the subsample including only workers with job tenure within 6 months of any threshold. The first column shows a significant effect of retirement on premature death for workers below the tenure threshold, while the third column only shows a small, and statistically, insignificant effect for workers just above the tenure threshold. A similar result is obtained if additional controls are used (compare columns (2) and (4)) and if the subsample considered includes workers within 12 months of any tenure threshold (remaining columns of Table 2.9).

Even though we cannot directly distinguish between voluntary and involuntary entry into early retirement, we find suggestive evidence that retirement voluntariness may indeed be related to the health effects of early retirement and the potentially underlying behavior. Early retirement followed by voluntary quits seem to be unrelated to mortality, while early retirement caused by involuntary layoffs is so.

## 2.8 Conclusions

This study estimates the causal effect of early retirement on mortality for blue collar workers. To resolve the problem of negative health selection into early retirement we exploit a policy change to the Austrian unemployment insurance system which allowed workers in eligible regions to withdraw permanently from employment up to 3.5 years earlier than workers in non-eligible regions. The program generated substantial exogenous variation in the effective early-retirement age: eligible male (female) blue collar workers retired on average 9 (12) months earlier than their non-eligible colleagues. This provides us with an empirical design which

Table 2.9: Retirement (in)voluntariness, individuals close the severance-pay threshold

Window around threshold	Death before 67					
	1 to 6 months			1 to 12 months		
	Below threshold	Above threshold		Below threshold	Above threshold	
Mean	0.1862	0.1862	0.1811	0.1899	0.1899	0.1622
Standard deviation	0.3894	0.3894	0.3852	0.3923	0.3923	0.3687
Retirement years before age 65	0.0225 (0.0181)	0.0334* (0.0189)	0.0104 (0.0182)	0.0194 (0.0145)	0.0314* (0.0165)	0.0190 (0.0145)
Cohort fixed-effects	Yes	Yes	Yes	Yes	Yes	Yes
Experience	Yes	Yes	Yes	Yes	Yes	Yes
NUTS-3 fixed-effects	Yes	Yes	Yes	Yes	Yes	Yes
Additional controls	No	Yes	Yes	No	Yes	Yes
Number of observations	1,332	1,332	1,458	2,938	2,938	2,866
R <sup>2</sup>	0.0890	0.1176	0.0614	0.0605	0.0823	0.0779

Notes: \*\*\*, \*\*, \* denotes statistical significance at the 1%, 5%, and 10% level respectively. Robust standard errors in parentheses. There are 25 (15) distinct male (female) cohorts, 10 controls for past work experience before age 50, and 8 distinct NUTS-3 regions. Additional control variables are the log of the average of yearly earnings between ages 43 and 49, the standard deviation of yearly earnings between ages 43 and 49, the number of sick-leave days before age 50 (10 terms), and employers' industry affiliation (14 industries).

allows us to identify the causal impact of early retirement on mortality using instrumental variable techniques.

For male blue collar workers, we find that early retirement age causes a significant increase in the risk of premature death (death before age 67). The effect for males is not only statistically significant but also quantitatively important. One additional year in early retirement causes an increase in the risk of premature death of 2.4 percentage points (a relative increase of 13.4%). Our results suggest that lower earnings of early retirees cannot explain male excess mortality because these losses are quantitatively too small to have a substantial impact on mortality. In contrast, we find that changes in health-related behavior (in particular, smoking and excessive alcohol consumption) contribute to a large extent to excess mortality. Male excess mortality is concentrated among three causes of deaths: (i) ischemic heart diseases (mostly heart attacks), (ii) diseases related to excessive alcohol consumption, and (iii) vehicle injuries. These three causes of death account for 78 percent of the causal retirement effect (while accounting for only 24 percent of all deaths in the sample). 32.4 percent of the causal retirement effect is directly attributable to smoking and excessive alcohol consumption. Our empirical results also suggest that early retirement following an involuntary job loss is likely to cause excess mortality among blue collar males, while retirement after a voluntary quit does not.

While the retirement-effect on mortality is highly significant and quantitatively important for males, we do not find such an effect for females. There are several reasons why male but not female workers suffer from higher mortality following early retirement. Women may be more able to cope with major life events, they may be more health-conscious and adopt less unhealthy behaviors; they may be more active due to their higher involvement in household activities; and they may suffer less from a loss of social status and identity.

In line with prior expectations and previous evidence, we also find that IV-estimates are smaller than the simple OLS estimate, both for men and for women. This is consistent with negative health selection into retirement and underlines the importance of a proper identification strategy when estimating the causal impact on mortality.

Our results have an obvious policy implication. From a welfare point of view, our results suggest that early retirement has severe negative welfare consequences for male blue collar workers. Increasing the effective early retirement age is therefore warranted not only because it helps to resolve the financing problems of pay-as-you-go social security systems but also because it increases individual welfare. Increasing life expectancy by raising the effective retirement age, however, will not help to resolve the financing problem one-for-one because increases in life expectancy will partly offset the improvement in the worker-retiree ratio.

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## 2.A Additional Tables and Figures

Table A.1: First stage results for cohorts ineligible to the REBP

	Men	Women
Mean	8.2685	5.3875
Standard deviation	3.4948	2.1897
Eligible district	−0.0071 (0.1115)	0.0791 (0.0800)
Cohort fixed-effects	Yes	Yes
Experience	Yes	Yes
NUTS fixed-effects	Yes	Yes
Additional controls	Yes	Yes
Number of Observations	3,444	3,005
R <sup>2</sup>	0.2397	0.1876

Notes: \*\*\*, \*\*, \* denotes statistical significance at the 1%, 5%, and 10% level respectively. Robust standard errors in parentheses. Considered birth cohorts are 08.1943–04.1947 for men and 08.1943–04.1952 for women. There are 25 (15) distinct male (female) cohorts, 10 controls for past work experience before age 50, and 8 distinct NUTS-3 regions. Additional control variables are the log of the average of yearly earnings between ages 43 and 49, the standard deviation of yearly earnings between ages 43 and 49, the number of sick-leave days before age 50 (10 terms), and employers' industry affiliation (14 industries).

Table A.2: The association between earnings from age 50 onwards and early retirement

	Earnings from age 50 onwards	
Mean	9.7237	9.7237
Standard deviation	0.3540	0.3540
Retirement years before age 65	−0.0222*** (0.0011)	−0.0250*** (0.0010)
Cohort fixed-effects	Yes	Yes
Experience	Yes	Yes
NUTS-3 fixed-effects	Yes	Yes
Additional controls	No	Yes
Number of observations	17,590	17,590
R <sup>2</sup>	0.3141	0.6223

Notes: \*\*\*, \*\*, \* denotes statistical significance at the 1%, 5%, and 10% level respectively. Robust standard errors in parentheses. Mean earnings derived from work income, unemployment benefits (assuming a replacement rate of 40%), and disability and old-age retirement (assuming a replacement rate of 80%) are estimated up to individuals' death date (right-censored death dates (July 1, 2009) are replaced by the expected death date based on workers' expected life-expectancy (taken from mortality tables by Statistics Austria). There are 25 (15) distinct male (female) cohorts, 10 controls for past work experience before age 50, and 8 distinct NUTS-3 regions. Additional control variables are the log of the average of yearly earnings between ages 43 and 49, the standard deviation of yearly earnings between ages 43 and 49, the number of sick-leave days before age 50 (10 terms), and employers' industry affiliation (14 industries).



Table A.3: Classification of alcohol- and smoking-related causes

Category	Included diseases <sup>a</sup>	ICD-9 <sup>b</sup>	ICD-10 <sup>b</sup>	Attributable fraction (in %) <sup>c</sup>
Alcohol-related causes	<u>Chronic conditions:</u>			
	Alcoholic psychosis	291	F10.3-F10.9	100
	Alcohol abuse	305.0, 303.0	F10.0, F10.1	100
	Alcohol dependence syndrome	303.9	F10.2	100
	Alcohol polyneuropathy	357.5	G62.1	100
	Degeneration of nervous system due to alcohol	n/a	G31.2	100
	Alcoholic myopathy	n/a	G72.1	100
	Alcohol cardiomyopathy	425.5	I42.6	100
	Alcoholic gastritis	535.3	K29.2	100
	Alcoholic liver disease	571.0-571.3	K70-K70.4, K70.9	100
	Alcohol-induced chronic pancreatitis	n/a	K86.0	100
	Liver cirrhosis, unspecified	571.5-571.9	K74.3-K74.6, K76.0, K76.9	40
	Esophageal cancer	150	C15	40
	Chronic pancreatitis	577.1	K86.1	84
	Portal hypertension	572.3	K76.6	40
	Gastroesophageal hemorrhage	530.7	K22.6	47
	<u>Acute conditions:</u>			
	Alcohol poisoning	980.0-980.1, E860.0-E860.1, E860.2, E860.9	X45,Y15, T51.0-T51.1, T51.9	100
	Suicide by and exposure to alcohol	n/a	X65	100
	Excessive blood level of alcohol	790.3	R78.0	100
Smoking-related causes	<u>Malignant Neoplasms:</u>			
	Lip, Oral Cavity, Pharynx	140-149	C00-C14	71
	Esophagus	150	C15	72
	Larynx	161	C32	82
	Trachea, Lung, Bronchus	162	C33-C34	87
	Urinary Bladder	188	C67	46
	<u>Cardiovascular Diseases:</u>			
	Aortic Aneurysm	441	I71	64
	<u>Respiratory Diseases:</u>			
	Bronchitis, Emphysema	490-492	J40-J42, J43	91
	Chronic Airway Obstruction	496	J44	81

Notes: <sup>a</sup> The choice of included diseases for alcohol-related causes is based on the Alcohol-Related Disease Impact (ARDI) software provided by the Centers for Disease Control and Prevention (CDC), one of the major operating components of the U.S. Department of Health and Human Services (HHS). We restrict alcohol-related diseases to those with alcohol-attributable mortality fractions of at least 40% (fractions of at least 40% are considered “high causation” diseases by the HHS). The alcohol-attributable mortality fractions refer to 5-year average annual estimates of health impacts based on the years 2001–2005 for U.S. males. The choice of included diseases for smoking-related causes is based on the Smoking-Attributable Mortality, Morbidity, and Economic Costs (SAMMEC) application also provided by the CDC. Again, we restrict smoking-related diseases to those with smoking-attributable mortality fractions of at least 40%. The smoking-attributable mortality fractions refer to U.S. males aged 65 and above in the year 2001. <sup>b</sup> ICD (International Classification of Diseases) is the international standard diagnostic classification for all general epidemiological, many health management purposes and clinical use. <sup>c</sup> Alcohol- or smoking-attributable fractions are defined as the proportion of deaths from the listed causes that are due to alcohol or smoking, respectively (these fractions are derived from meta-studies conducted by the HHS).

Table A.4: Causes of death, disaggregation of *other causes* (see column (6) of table 2.8), men only

	Other causes	Alcohol-unrelated digestive system diseases	Non-ischemic heart diseases	Smoking-unrelated respiratory diseases	Smoking-unrelated cancer	Self-inflicted injuries	Other injuries	Cerebro-vascular diseases	All remaining causes
Mean	0.1101	0.0020	0.0125	0.0019	0.0308	0.0049	0.0036	0.0067	0.0476
Standard deviation	0.3130	0.0446	0.1111	0.0439	0.1728	0.0702	0.0602	0.0813	0.2130
Retirement years before age 65	0.0079 (0.0072)	-0.0011 (0.0009)	0.0039 (0.0026)	0.0000 (0.0010)	0.0028 (0.0039)	0.0020 (0.0018)	-0.0003 (0.0015)	0.0014 (0.0019)	-0.0009 (0.0050)
Cohort fixed-effects (biannually)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Experience	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
NUITS fixed-effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Additional controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	17,590	17,590	17,590	17,590	17,590	17,590	17,590	17,590	17,590
R <sup>2</sup>	0.0292	.	0.0075	0.0062	0.0059	0.0058	0.0038	0.0066	0.0102

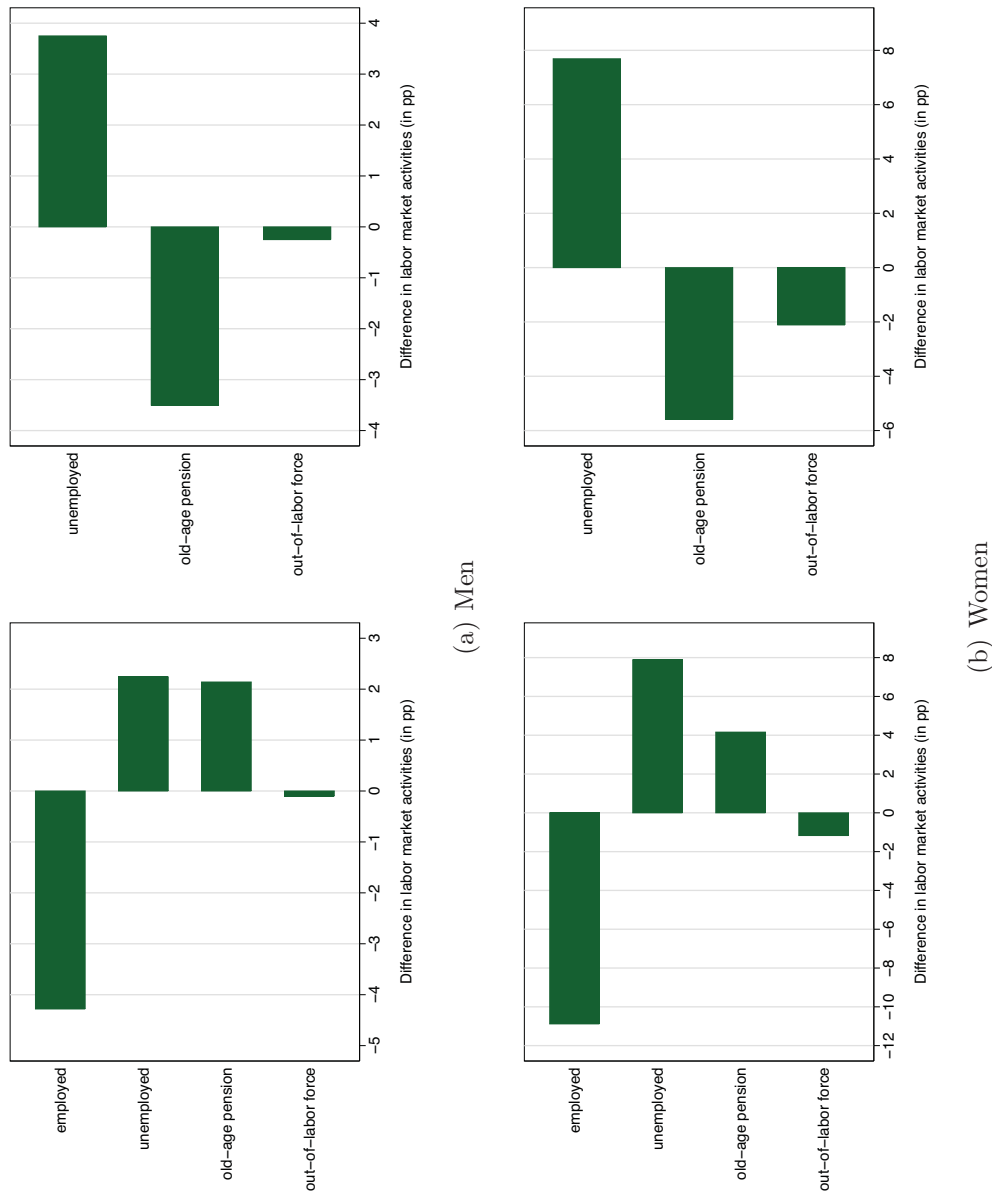
Notes: \*\*\*, \*\*, \* denotes statistical significance at the 1%, 5%, and 10% level respectively. Robust standard errors in parentheses. A missing R<sup>2</sup> denotes a negative R-squared. The causes of death are classified by means of ICD-9 and ICD-10. "Other causes" are defined as described in the notes of Table 2.8. Columns (2)–(9) disaggregate the diseases included in the category "other causes" in column (6) of Table 2.8 into more specific subcategories. The ICD codes for the subcategories are as follows: *Alcohol-unrelated digestive system diseases*: 520-579 (ICD-9); K00-K93 (ICD-10). *Non-ischemic heart diseases*: 390-429, 440-459 (ICD-9); I01-I52, I70-I99 (ICD-10). *Smoking-unrelated respiratory diseases*: 460-519 (ICD-9); J00-J99 (ICD-10). *Smoking-unrelated cancer*: 140-239 (ICD-9); C00-D48 (ICD-10). *Self-inflicted injuries*: 950-959 (ICD-9); X60-X84 (ICD-10). *Other injuries*: 850-869, 880-949 (ICD-9); W00-W99, X01-X59 (ICD-10). *Cerebrovascular diseases*: 430-438 (ICD-9); I60-I69 (ICD-10). *All remaining causes* include the remaining causes as well as those observations for which the cause of death is unknown due to failure of the match between data on causes of death and the ASSD. Note, however, that these subcategories necessarily exclude those diseases that are contained in the "alcohol- and smoking-related causes" as defined in Table 2.8. Hence, these subcategories are, taken together with the categories in columns (2)–(5) of Table 2.8, exhaustive and mutually exclusive.

Table A.5: Health predisposition

Sick leave days (past 10 years)	Death before age 67			
	Below median		Above median	
Mean	0.1481	0.1481	0.2117	0.2117
Standard deviation	0.3553	0.3553	0.4085	0.4085
Retirement years before age 65	0.0072 (0.0110)	0.0122 (0.0118)	0.0254** (0.0109)	0.0340*** (0.0120)
Cohort fixed-effects	Yes	Yes	Yes	Yes
Experience	Yes	Yes	Yes	Yes
NUTS fixed-effects	Yes	Yes	Yes	Yes
Additional controls	No	Yes	No	Yes
Number of observations	8,681	8,681	8,909	8,909
R <sup>2</sup>	0.0267	0.0399	0.0722	0.0845

Notes: \*\*\*, \*\*, \* denotes statistical significance at the 1%, 5%, and 10% level respectively. Robust standard errors in parentheses. There are 25 (15) distinct male (female) cohorts, 10 controls for past work experience before age 50, and 8 distinct NUTS-3 regions. Additional control variables are the log of the average of yearly earnings between ages 43 and 49, the standard deviation of yearly earnings between ages 43 and 49, the number of sick-leave days before age 50 (10 terms), and employers' industry affiliation (14 industries).

Figure A.1: Labor-market activity after age 50 and since entry into retirement, respectively



Notes: The figures show differences in specific labor-market activities between eligible and non-eligible districts (in percentage points), for men and women separately. The figures on the left show differences in activity from age 50 until min(age 65, death); the figures on the right show differences in activity from the effective retirement age until min(age 65, death).



## CHAPTER 3

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### Do Financial Incentives Affect Firms' Demand for Disabled Workers?

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Joint with Rafael Lalive and Josef Zweimüller

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#### 3.1 Introduction

Integrating disabled workers is a key challenge of employment policy. One out of seven individuals who live in OECD countries report a health problem that limits activities of daily life (OECD, 2003). Employment matters tremendously for disabled individuals' economic well-being. The work incomes of disabled individuals with a job are nearly as high as those of individuals without a disability. In contrast, the financial resources available to a disabled individual without a job are 46 % lower than the disposable income of an employed disabled individual. Even though work is of crucial importance for disabled individuals' material standard of living, their employment rates are substantially below those of the non-disabled.

This chapter studies whether an employment quota for firms can help increase the demand for disabled workers. Understanding the effects of quota is important for several reasons. First, the two most important policies for encouraging employment of disabled workers among OECD member countries are anti-discrimination legislation and employment quotas. While the effects of anti-discrimination policies are quite well understood, the effects of employment quotas on firms' employment decisions have been explored less. Second, labor economists

have long attempted to understand the importance of financial incentives in labor demand (Hamermesh, 1993). The employment quota policy allows studying firms' reaction to a sharp change in the relative cost of employing disabled and non-disabled workers. Third, legislation very similar to that in Austria is in force in many other OECD countries (or has been so until very recently, as in the U.K). (table 3.1 provides an overview). While these regulations have a core component in the form of a mandatory employment quota in common, they differ in terms of the quota amount (ranging from 7% in Italy to 2% in Korea and Spain), in terms of the target firms, and in terms of the salience of non-compliance sanctions (ranging from 0.25% of the monthly pay-roll for firms in Germany to 4% in Italy).

Table 3.1: OECD countries with employment quotas

Country	Quota	Targeted firms	Sanction
Austria	4%	private and public employers with over 25 employees	€ 200.- per month for each place not filled (0.4% of payroll)
Belgium	2–2.5%	only public employers	–
France	6%	public and private employers with over 19 employees	€ 150–250 per month (0.45–0.75% of payroll)
Germany	5%	public and private employers with over 19 employees	€ 100–250.- per month for each place not filled, depending on fulfillment (0.25–0.65% of payroll)
Italy	7%	public and private employers with over 50 workers, one/two places for 15–35/36–50 employees	€ 1,075.- per month for each place not filled (4% of payroll)
Korea	2%	public sector and private employers with over 300 employees	€ 324.- per month for each place not filled (0.5% of payroll)
Poland	6%	public sector and private employers with over 50 employees	40.65% of average wage per month for each place not filled (2.4% of payroll)
Spain	2%	public sector and private employers with over 50 employees	–

Source: OECD (2003)

We study the case of Austria, where the Disabled Persons Employment Act (DPEA) defines specific employment targets, coupled with financial incentives for meeting these targets. Austrian law firms have to hire at least one disabled individual per 25 non-disabled employees. Firms that fail to comply with this obligation are subject to a tax of currently 223 € per month, i.e. about 12 % of the average wage of Austrian employees or 0.48 % of the wage bill for 25 non-disabled workers. The tax revenues are used to subsidize firms that provide employment to disabled workers (regardless of whether they are subject to the employment quota or not).

We propose an empirical strategy that exploits the discontinuous change in financial incentives due to employment quota. To assess the role of the non-compliance tax on threshold

firms employing 25 (or 50, 75, etc.) non-disabled workers, we compare the number of disabled workers in firms just below and just above the quota threshold. The central idea of this empirical strategy is this: When a firm with 25 non-disabled workers decides not to hire a disabled worker, it has to pay the non-compliance tax. In contrast, when a firm with 24 non-disabled workers does not hire a disabled worker, it is not subject to this tax.

The objective of our empirical analysis is to estimate the effect of the non-compliance tax on disabled employment for threshold firms if they did not have to pay the non-compliance fine at the current threshold but a slightly modified threshold. Note that the counterfactual is not a world without a quota system. Rather, we focus on how the quota's non-compliance tax affects the marginal firms' disabled employment decisions. This effect is policy relevant for at least two reasons. First, it is central to understand whether financial sanctions work for the marginal firm before assessing the overall employment effects. It is hard to see how the overall employment effect could be non-zero if the marginal firms are not affected. Second, the causal parameter we study is central in calculating the effects of marginal changes to the quota amount on disabled employment. It is crucial to understand this parameter because the quota level is both the most important parameter of the policy and it varies strongly across OECD countries.

The key threat to a causal interpretation of our results is that firm size is endogenous. Firms may self-select below thresholds to avoid becoming subject to the employment quota. Manipulation of threshold location invalidates the regression discontinuity design (McCrary, 2008).<sup>1</sup> We address manipulation in two ways. First, we develop a simple behavioral framework to assess whether self-selection takes place. The key result of this theoretical analysis is that i) self-selection need not take place for low non-compliance taxes and ii) if manipulation takes place our approach will produce an estimate of a lower bound on the causal parameter. Second, we implement two important checks to detect manipulation. First, endogenous self-selection is expected to result in a discontinuity in the firm size distribution. Our empirical evidence indicates that there is no such discontinuity. Second, the two populations of firms below and above thresholds are very similar in terms of a range of observable characteristics. Thus, both the theoretical analysis of manipulation and the empirical checks for it suggest that it is not present. We thus maintain the central identifying assumption that firms just below thresholds provide valid information on the employment decisions of threshold firms without the quota system. The causal impact of employment quota can therefore be identified by comparing employment decisions of threshold firms to those of firms just below thresholds.

The empirical analysis documents four important results. *First*, firms facing the obligation to employ disabled workers do in fact employ more disabled workers than similar firms without

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<sup>1</sup>The RDD has been used in a number of studies to measure causal effects. See Angrist and Lavy (1999), DiNardo and Lee (2004), Imbens and Lemieux (2008), and Lalive (2008b), for studies assessing the causal effects of unions, social assistance, or unemployment benefits on labor market outcomes.



this obligation. A comparison of firms just above the quota threshold to those just below the threshold shows that roughly 1 in 25 firms around the first threshold (25 non-disabled workers) have a disabled worker on the payroll whom they would have not hired in the absence of the employment quota. The average effect at higher order thresholds (50, 75, ...) is roughly twice as large but imprecisely estimated. Both estimates suggest that firms are quite responsive to the tax (the elasticity of substitution is around 1.60 for firms employing 25 non-disabled workers and around 1.58 for firms hiring 100 non-disabled workers).<sup>2</sup> *Second*, we document important heterogeneity of the effects of employment quota with respect to wages. We find that firms' response to the per-head non-compliance tax decreases monotonically with a firm's position in the wage distribution. *Third*, we explore the extent to which firms' employment decisions merely reflect poaching from other firms rather than creating or maintaining employment. We find that roughly 64 % of the employment effect can be attributed to workers already employed by the firm on the date of acquiring formal disability status. About 34 % of excess employment can be attributed to workers who were employed by other firms at the time of acquiring disability status. The remaining 2 % of excess employment goes to individuals who were not employed at the time of acquiring disability status. *Fourth*, two reforms of the system suggest that increasing the non-compliance tax increases excess disabled employment, whereas paying a bonus to over-complying firms slightly dampens the employment effects of the non-compliance tax.

The existing literature has extensively studied the effects of anti-discrimination legislation for disabled individuals. Using state-by-state variation in the timing of passage of the Americans with Disabilities Act (ADA), DeLeire (2000), Acemoglu and Angrist (2001), and Beegle and Stock (2003) find that the ADA has not improved employment of disabled individuals in the U.S. and may, in some cases, have even reduced their employment chances. Kruse and Schur (2003) challenge this finding, arguing that the data used in the earlier studies may not have provided precise information on disability status. Jolls and Prescott (2004) and Jolls (2004) argue that the ADA increased education participation by those individuals for whom the ADA probably offered improved employment prospects, and argue that increased education participation is the result of an increase in the return on further education. Bell and

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<sup>2</sup>Recall that the elasticity of substitution is the negative of the percentage change disabled to non-disabled employment caused by a percentage change in the relative disabled to non-disabled wage. Consider the first threshold. Disabled to non-disabled employment increases by 0.15 % (effect of 0.0373 divided by threshold firm size of 25) because the tax reduced the relative wage of a disabled worker by about 7.5 percent (regular monthly earnings are 1,850 EUR; this means that the tax decreases disabled to non-disabled relative earnings from 1,850 / 1,850 to 1,850 / 2,000). The relative disabled and non-disabled wage is 1 whereas the disabled to non-disabled employment level stands at 0.31/25. Thus the elasticity of substitution of threshold firms stands at about 1.60 ( $= -(0.0373/25)/(1850/2000 - 1) \cdot 1/(0.31/25)$ ). The corresponding elasticity for the threshold firm with 100 non-disabled workers is 1.58 ( $= -(0.0636/100)/(2000/2150 - 1) \cdot 1/((2.3044/4)/100)$ ). Note that average disabled employment is adjusted to reflect that the firm with 100 workers has already passed three thresholds and regular monthly earnings are 2,000 EUR rather than 1,850 EUR in large firms.

Heitmueller (2009) study the effects of the Disability Discrimination Act in the U.K. Their results confirm that, as in the U.S., disability legislation did not have a significant impact on employment prospects for disabled individuals in the U.K.<sup>3</sup> The existing literature on the effects of employment quota is rather sparse. Wagner *et al.* (2001) study employment quota in Germany.<sup>4</sup> Their paper assesses the impact of the employment quota in Germany on job dynamics in 400 small firms and finds no effect of the quota threshold. Wuellrich (2010) studies the employment effects of an increase in the non-compliance tax in Austria and finds a positive effect on employment of disabled workers.<sup>5</sup>

This chapter contributes to the literature in at least two dimensions. *First*, our study adds to the literature by discussing whether a regression discontinuity design can be used to estimate the causal effect of the employment quota on threshold firms' employment of disabled workers. While a large number of studies have looked at the effects of anti-discrimination legislation with respect to disabled workers, we are not aware of previous studies that attempt to evaluate the effect of quota rules on employment of disabled workers. While theoretical findings caution against adopting regression discontinuity for point identification, they suggest regression discontinuity provides useful estimates of the lower bound on the causal role of non-compliance taxes. Furthermore, the empirical checks for failures of regression discontinuity do not indicate significant departures from the key identifying assumption. The regression discontinuity design therefore appears a useful design to evaluate non-compliance taxation. *Second*, our evaluation is based on high-quality data from Austrian private firms and their (disabled and non-disabled) workers. In fact, we use the same data sources the Austrian social welfare authorities use to determine compliance with employment quota: the Austrian Social Security Data (ASSD) linked to data from the Austrian Federal Welfare Office (FWO). The former data set allows us to calculate the exact size of the labor force (divided into disabled and non-disabled workers) of every single Austrian firm. The latter data set allows us to assess the number of individuals with formal disability status within each firm. Since our data set covers all of the 46,467 Austrian private sector firms from 1996–2003 situated close to quota thresholds, we can provide informative contrasts of firms just below and just above the quota threshold to estimate the quota effect. (See Zweimüller *et al.*, 2009, for a description of the ASSD)

The chapter is organized as follows. Section 3.2 provides a detailed description of the institutional environment in Austria. Section 3.3 presents the behavioral framework. Section

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<sup>3</sup>See also Lechner and Vazquez-Alvarez (2009) and Verick (2004) who study the effects of German anti-discrimination legislation. Moreover, two strands of the literature study (i) the role of employment protection for worker effort (Ichino and Riphahn, 2005), and (ii) the role of general employment protection provisions on firm dynamics and firm size (Bertrand and Kramarz, 2002; Borgarello *et al.*, 2004).

<sup>4</sup>See Welch (1976) for an early theoretical attempt to characterize the effects of quota on the labor market.

<sup>5</sup>See also Humer *et al.* (2007) for an overview of the Austrian system and a descriptive account of disabled workers' career patterns.

3.4 describes the data and the empirical strategy. Our main results are presented in section 3.5. Section 3.6 concludes.

## 3.2 Background

This section provides a description of the key legal background document – The Disabled Persons Employment Act (DPEA). DPEA was implemented in Austria in 1970 and forms the legal basis of the Austrian employment quota system – its main instrument – to promote employment among severely disabled workers. It defines the process by which individuals acquire the formal status of being “severely disabled”, regulates the employment obligations for firms and the financial sanctions associated with non-compliance of these obligations, specifies rules on how to pay out subsidies to firms employing disabled workers, and introduces employment protection rules for disabled workers. We first discuss the legal background as it applied to the period January 1999 to June 2001. We then discuss two important reforms to this system that took place before and after this period.

**Employment quota** The quota rule obliges firms to hire one disabled worker per 25 non-disabled workers, leading to a quota of 4 %.<sup>6</sup> Firms that do not comply with this obligation are subject to a flat non-compliance tax. The non-compliance tax currently amounts to € 223 (2010) but it stood at about 150 € in 1999. The non-compliance tax amounted to roughly 8 % of a worker’s average monthly salary or 0.32 % of a firm’s average monthly payroll for 25 non-disabled workers.

The FWO is in charge of enforcing the employment obligation, by checking the size of each firm and the number of employed disabled workers on the first day of each month. The exact calculation of the employment quota takes the particular disabilities into account. There is some double-weighting, i.e. particular groups of disabled workers are equivalent to two disabled workers, which include the (i) blind, (ii) disabled individuals of age 19 years or younger, (iii) disabled apprentices, (iv) disabled individuals of age 50 or older with a degree of disability of at least 70 percent, (v) disabled individuals of 55 years or older, and (vi) individuals in a wheelchair. The FWO levies a non-compliance tax on firms that do not fulfill the employment quota. Disabled workers have to be hired on the same type of contracts offered to non-disabled workers, both with respect to wages and part-time vs. full-time status. This means that firms can not just temporarily employ a disabled worker to fulfill the quota.

Importantly, the non-compliance tax for disabled worker is the only labor market regulation that kicks in at a firm size of 25 non-disabled workers (and multiples thereof). This means

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<sup>6</sup>The Austrian quota is lower than that in Germany (5 %), France (6 %), Poland (6 %), and Italy (7%); but it is higher than that in Belgium, Korea, and Spain (2 %).

that contrasting firms with 25 employees to firms with fewer than 25 workers really informs on the non-compliance tax rather than on other labor market regulations.<sup>7</sup>

**Acquisition of disability status** The process by which working age individuals acquire the formal status of a disabled individual is as follows. In order to become entitled, disabled individuals have to file an application with the Austrian Federal Welfare Office (FWO). The application is approved once a FWO medical expert assesses a physical, mental, intellectual, or sensory disorder which reduces the individual's work capacity by at least 50 percent. This procedure aims to rule out that the formal status of a disabled individual can be obtained by fraud – at either the initiative of a worker himself nor of a firm putting pressure on one of his employees. We can not rule out that the procedure is only triggered after a firm has crossed the threshold. Yet we expect relabeling of existing workers to be of minor importance. Both workers and firms have important incentives to initiate the process to become recognized disabled at onset of disability. Workers gain in terms of increased job protection. Firms gain in getting access to accommodation and wage subsidies (see below).

Recognized disabled individuals make up a non-negligible proportion of the Austrian work force. In 2009, almost 95,000 individuals or 2.2 % of total employment were registered as disabled according to the law.

**Workplace accommodation and (wage) subsidies** The DPEA also defines how the revenues collected through non-compliance taxes are to be spent. These revenues amounted to € 88.2 millions in 2009. The main beneficiaries are those firms that actually offer employment to disabled workers as well as the disabled workers themselves. Firms are eligible for three kinds of allowances (in form of grants, loans, or benefits in kind) of the following kind.

First, allowances are granted for costs associated with the provision of adequate access to the premises and adequate workplace accommodation in favor of their disabled workers. This kind of allowance is limited to € 25,000, requires that the applying firm contributes 50% to the total costs involved, and is only available to firms with less or equal to 50 non-disabled employees.

Second, allowances are granted for wage subsidies. Wage subsidies accrue to four groups of workers: they accrue (i) to entrants in the amount of at most € 700 a month for up to two years, (ii) to current employees if the firm can show credibly the reduction in work capacity due to a impairment (limited to not exceed 50% of the disabled worker's monthly wage or € 650 a month), (iii) to apprentices in the amount of at most € 400 a month for the duration of the entire apprenticeship, and (iv) to disabled workers whose employer can show credibly that

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<sup>7</sup>There is a discontinuity in labor regulations at firm size of 15 employees. Firms above this employment threshold have to establish a works council.

their appointment is at risk without a wage subsidy (at most 50% of the disabled worker's monthly wage, but no more than € 1,000 a month, for up to three years).

Third, firms can apply for work assistance (such as counseling the firm regarding the efficient integration of disabled workers). This is service free of charge and provided by the FWO.

Basically, these allowances represent a reallocation of resources from firms that fail to comply with the quota rule to those firms that employ at least one disabled worker. The reallocation is used to compensate the latter for their effort in employing disabled workers. Note, however, that these allowances are available to all firms, not just to those subject to the employment obligation.<sup>8</sup>

Disabled workers are eligible to allowances for the following purposes: vocational (re)training, professional development, work assistance (counseling service), mobility enhancing measures (e.g. provision of a guide dog), and formation of a subsistence securing self-employment (up to € 60,000).

**Employment protection** The DPEA provides increased employment protection for disabled workers, i.e. protection from dismissal and protection from wage cuts due to disability. The increased protection against dismissal is twofold. First, it stipulates that a contract may only be terminated after a notice period of at least four weeks. Second, dismissal is only valid if a special FWO committee agrees to it. Dismissals without the consent of this committee are unlawful. However, the increased dismissal protection comes into effect only after a probationary period of three months has elapsed.

**Policy Changes** There have been two important reforms of the DPEA since the late 1990s. First, the non-compliance tax was subject to an extraordinary increase by € 46 or 30% on July 1, 2001, from € 150 to € 196.<sup>9</sup> Second, the DPEA originally included a bonus for firms that employed more disabled workers than they actually had to according to the employment quota. Over-complying firms were granted a bonus in the amount of the non-compliance tax for each excess disabled worker per month. The bonus for over-compliance was abolished on January 1, 1999. Third, the probationary period originally amounted to one month, and was extended in two steps: from one to three months on January 1, 1999 and from three to

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<sup>8</sup>Dyk *et al.* (2002) investigate, *inter alia*, to what extent firms make use of these allowances. They conclude from their firm survey that almost 40% of firms that employ at least one disabled worker receive such allowances. Wage subsidies constitute the most important type of allowance (86% of those firms that claim some form of allowances obtain wage subsidies). This number only amounts to 6% for allowances regarding costs associated with the provision of adequate access to the premises and adequate workplace accommodation in favor of their disabled workers. Moreover, 60% of all firms assess the existence of such allowances as essential for the hiring of disabled workers.

<sup>9</sup>The non-compliance tax is indexed to the inflation rate.

currently six months on July 1, 2001.

In the empirical analysis of the DPEA (see section 3.5), we focus on the period from January 1999 to June 2001, but will provide results also for the preceding and subsequent period to assess the role of changes in the salience of the tax and the role of combining the tax with a bonus.

### 3.3 A Simple Behavioral Framework

This section discusses the firm's decision problem in a simple framework designed to discuss whether manipulation of non-disabled firm size takes place in a quota system with non-compliance taxation. This discussion is central in order to understand whether or not our proposed identification strategy works.

**Set-up** Firms can hire two types of workers: disabled and non-disabled. Non-disabled workers have productivity  $P$  and disabled workers have productivity  $p < P$ . Labor is indivisible and can be hired only in discrete amounts. We focus on local employment decisions, i.e. employment decisions of firms hiring at or close to the first quota threshold  $T = 25$  and explore which firms will locate around that threshold.<sup>10</sup> Thus, let  $S = 0, 1, 2, \dots$  denote the number of non-disabled and  $Y = 0, 1, 2, \dots$  the number of disabled workers hired by the firm. We distinguish between employment choices made in the absence of the quota tax, denoted by  $S_0$  and  $Y_0$ , and employment choices with quota tax, denoted by  $S_1$  and  $Y_1$ . Manipulation of non-disabled employment refers to a situation where  $S_1 \neq S_0$ .

We assume that both types of workers earn the same real wage  $w$  despite their productivity not being identical.<sup>11</sup> The wage does not fall short of the productivity of the disabled worker, i.e.  $0 < w < p < P$ , so the firms' real profit per disabled worker is positive.<sup>12</sup>

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<sup>10</sup>Note that productivity per worker is not assumed to be globally independent of firm size and composition of the workforce, i.e.  $P$  and  $p$  differ between firms hiring a total of 10 workers compared to firms hiring a total of 100 workers. Technology shapes global firm size and composition. Our framework can be understood to describe how firms integrate small differences in local demand condition into their employment choices for firms whose technology delivers a firm size around the quota threshold  $T$ . Also note that extending the analysis to higher order thresholds would follow similar logic as we discuss below. We therefore concentrate on explaining the consequences of manipulation at the first quota threshold  $T = 25$ .

<sup>11</sup>Firms can not cut wages of workers who experience a sudden decrease in work capacity. Moreover, our analysis of wage determination of disabled and non-disabled workers is consistent with this assumption being plausible also for new entrants.

<sup>12</sup>The assumption that non-disabled workers produce more than they cost may appear in contrast with the fact that recognized disabled workers must have lost at least 50 % of work capacity. Note, however, that firms have access to wage subsidies and subsidies for workplace accommodations. Also, firms care about the effective wage, i.e. the wage net of wage subsidies. Wage subsidies are likely to contribute to disabled workers being profitable. We do not explicitly take wage subsidies into account noting that doing so would be relatively straightforward.



Our key focus is on firms' employment decisions rather than on pricing behavior and general equilibrium interactions. For simplicity, we assume that firms take real profits per worker as given ( $1 - w/p$  per disabled worker; and  $1 - w/P$  per non-disabled worker) and make their employment decisions after they have learned the level of demand for their product, denoted by  $Z$ . Demand is continuously distributed across firms.

**Employment without tax** Without the quota in place, firms maximize

$$\pi_0(S, Y) = \min(SP + Yp, Z) - (S + Y)w$$

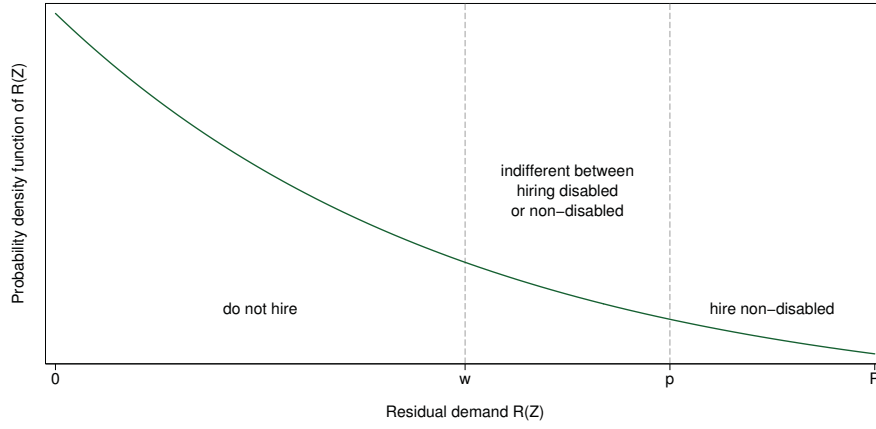
Consider first optimal employment of non-disabled workers (assuming firms do not hire disabled workers). Optimal non-disabled employment is characterized by a threshold rule. All firms will hire at least  $Z/P$  non-disabled workers rounded to the integer below, i.e. the "floor" number of non-disabled workers  $\lfloor Z/P \rfloor$ . Firms will hire an additional non-disabled worker if and only if the residual demand for their product exceeds the wage rate. Thus, let  $R(Z) \equiv Z - \lfloor Z/P \rfloor P$  denote residual demand. Firms will hire  $Z/P$  rounded to the next integer above, the "ceiling" number of workers, if and only if  $R(Z) > w$ .

Consider now optimal hiring of disabled workers. Clearly, firms who do not hire the marginal non-disabled worker also will not hire a disabled worker. These firms have low residual demand  $0 < R(Z) < w$ . Yet firms with intermediate residual demand, i.e.  $w < R(Z) < p$ , are indifferent between employing one non-disabled worker or one disabled worker. Both will produce extra revenue of  $w < R(Z) < p$  and cost the same. We assume that these firms do employ an extra disabled worker. Firms with high residual demand, i.e.  $p < R(Z) < P$ , do not hire a disabled worker since hiring a non-disabled worker increases revenue by more than hiring a disabled worker.

Figure 3.1a summarizes firms' optimal employment choices *without* the quota in place by displaying the probability density function of the residual demand  $R(Z)$ . We assume an exponential residual demand distribution. Figure 3.1a shows that the majority of firms face residual demand  $R(Z) \in [0, w]$ . Such firms stay at the floor number of non-disabled employment without hiring a disabled worker. Firms facing residual demand  $R(Z) \in [w, p]$  hire an additional worker, thereby being indifferent between a disabled or non-disabled one. The remaining firms face residual demand  $R(Z) \in [p, P]$ . They employ the ceiling number of non-disabled workers (and no disabled workers).

**Employment with non-compliance taxes** Now suppose there is a system that requires firms with non-disabled employment  $S \geq T$  to hire one disabled worker whereas firms with  $S < T$  do not face such an obligation. To enforce compliance with the employment quota, firms that do not hire the disabled worker have to pay a tax  $\tau$ . We assume that the tax is

Figure 3.1: Employment choices derived from the model



(a) optimal employment choices without taxes



(b) optimal employment choices with taxes

lower both than the wage rate  $\tau < w$  and the productivity differential between disabled and non-disabled workers, i.e.  $\tau < P - p$  or  $p < P - \tau$ .<sup>13</sup>

With quota in place, firms maximize

$$\pi_1(S, Y) = \min(SP + Yp, Z) - (S + Y)w - \min(\lfloor S/T \rfloor - Y, 0)\tau$$

How does the tax affect employment decisions? Consider firms who would hire below the threshold without the tax, i.e. firms with  $S_0 < T$ . Non-compliance taxes are irrelevant for this set of firms, so employment choices are not affected, i.e.  $S_1 = S_0$  and  $Y_1 = Y_0$ .

Next, consider firms who would hire exactly at the threshold if the tax were not present,

<sup>13</sup>Both assumptions are well in line with the Austrian system where the tax is on the order of 8 % of monthly earnings. Moreover, productivity of a disabled worker is likely to be substantially lower than productivity of a non-disabled worker since disabled individuals have lost at least 50 % of work capacity.



i.e.  $S_0 = T$ . How these firms react to the tax depends on residual demand.

Consider firms with high residual demand, i.e.  $p < R(Z) < P$ . Some of these firms will substitute a non-disabled worker by a disabled worker because doing so increases profit by the tax  $\tau$  and reduces profit by  $R(Z) - p$ .<sup>14</sup> This means that firms with  $p < R(Z) < p + \tau$  will substitute one non-disabled worker with a disabled worker, i.e.  $S_1 = S_0 - 1$  and  $Y_1 = Y_0 + 1$ . These firms manipulate non-disabled employment to avoid paying the tax. The firms with large residual demand, i.e.  $p + \tau < R(Z) < P$ , will not change employment in response to the tax, i.e.  $S_1 = S_0$  and  $Y_1 = Y_0$ .

Next, consider firms with intermediate residual demand, i.e.  $w < R(Z) < p$ . These firms employ a disabled worker as per our assumption above. Introducing the tax does not change their decisions, i.e.  $S_1 = S_0$  and  $Y_1 = Y_0$ .

Finally, consider firms with low residual demand, i.e.  $0 < R(Z) < w$ . Some firms in this group are interested in hiring a disabled worker because doing so saves the tax  $\tau$  and costs  $w - R(Z)$ . This means that firms with residual demand  $w - \tau < R(Z) < w$  employ one extra disabled worker, i.e.  $Y_1 = Y_0 + 1$ , and the same number of non-disabled workers, i.e.  $S_1 = S_0$ . Firms with really low residual demand  $0 < R(Z) < w - \tau$  are not affected by the tax, i.e.  $S_1 = S_0$  and  $Y_1 = Y_0$ .

Figure 3.1b summarizes firms' optimal employment choices *with* the quota in place. In contrast to figure 3.1a, firms that face residual demand as low as  $R(Z) \in [w - \tau, w]$  no longer stay at the floor number of non-disabled workers, but hire an extra disabled worker. Moreover, firms facing residual demand  $R(Z) \in [w, p]$  are not indifferent between hiring a disabled or non-disabled worker anymore. They now unambiguously opt for the disabled one. Finally, while firms facing residual demand  $R(Z) \in [p, p + \tau]$  hired an additional non-disabled worker *without* the quota in place, they now substitute this worker by a disabled one.

**Consequences of Manipulation** The presence of the quota leads some firms to choose firm size just below the quota threshold that would not choose that firm size in the absence of the quota. How does manipulation a Regression Discontinuity estimate (RDE) of the role of employment quota? Simply put, the RDE is based on contrasting firms who choose to hire at the threshold  $T$  to firms who choose to hire just below the threshold (we discuss the details of the empirical strategy further below). The key question is whether and to what extent manipulation biases mean employment of disabled workers by threshold firms.

Let observed non-disabled employment be  $S$ . Note that  $S = S_0$  for firms that are not affected by the presence of the tax (i.e.  $S_0 < T$ ) and  $S = S_1$  for firms that are affected by the presence of the tax (i.e.  $S_0 \geq T$ ). Suppose that residual demand  $R(Z)$  conditional on "floor"

<sup>14</sup>Note that none of these firms will simply let go the last non-disabled worker since that costs  $R(Z) - w$  but saves only  $\tau$ , i.e. this choice is strictly dominated by simply substituting the last non-disabled worker by a disabled worker.

non-disabled employment is distributed according to a distribution  $G(r, z) \equiv \text{Prob}(R(Z) < r | [Z/P] = z)$ . Assume that  $G(r, z)$  is independent of "floor" non-disabled firm size, i.e.  $G(r, z) = G(r)$  in a small neighborhood around the quota threshold  $T$  with standard properties ( $G(0) = 0$  and  $G(P) = 1$ ).

Consider firms hiring at the threshold with observed firm size  $S = T$ . This set of firms consists of two sub-groups. The non-manipulators are the firms with residual demand  $0 < R(Z) < p$  and  $p + \tau < R(Z) < P$  and non-disabled employment of  $S = S_1 = S_0$ . The manipulators are the firms with residual demand  $p < R(Z) < p + \tau$  and  $S = S_1 = S_0 - 1$ . These firms want to employ one non-disabled worker without the tax but choose to substitute that worker by a disabled worker due to the tax. Manipulation does not increase the number of firms located at the threshold, i.e. the set of firms located at the threshold is  $G(P) = 1$ . Interestingly, manipulators entering the non-disabled group at  $T$  from  $T + 1$  replace the manipulators leaving non-disabled employment  $T$  for  $T - 1$ . This means that – on average – the number of disabled workers hired by threshold firms is *identical* with manipulation as it would be without manipulation.

Consider firms hiring just below the threshold with observed firm size  $S = T - 1$ . These firms serve to identify the counterfactual disabled employment decisions without tax. Again, this set of firms consists of two groups. The first group is the population of firms choosing to employ  $S = S_0 = T - 1$  non-disabled workers. The second group is the set of manipulating firms, i.e. firms with  $p < R(T) < p + \tau$  and  $S_1 = T - 1$  but  $S_0 = T$ . Thus, manipulation increases the number of firms located just below the threshold from 1 to  $1 + G(p + \tau) - G(p)$  – manipulation creates bunching (McCrary, 2008). Bunching is weak if the tax is small or there are few firms with residual demand between  $p$  and  $p + \tau$ . Moreover, manipulation introduces an upward bias in mean disabled employment since all manipulating firms hire a disabled worker because of the tax but would not without the tax.

In sum, this simple framework suggests that manipulation of non-disabled firm size may take place, manipulation can be detected with a simple test for bunching, and manipulation introduces an upward bias into mean disabled employment just below the quota threshold. This discussion implies that regression discontinuity generally produces a lower bound on the causal effect of the non-compliance tax on disabled employment. Moreover, manipulation can be detected and serves to assess the key identifying assumption that firms just below the threshold inform on the counterfactual for firms at the threshold.

### 3.4 Empirical Strategy

The first part of this section provides the essential background regarding the data for the empirical analysis. The second part of this section discusses identification and estimation of

the causal effect of the non-compliance tax on disabled employment.

### 3.4.1 Data

To assess the impact of the employment quota on the firms' hiring decisions with respect to disabled workers, we use register data from two different sources: (i) the Austrian Social security database (ASSD), which contains detailed information on the individuals' employment history and characteristics from 1972–2003 on a daily basis together with an unambiguous firm identifier, as well as firms' industry affiliation and location (see Zweimüller *et al.*, 2009) and (ii) personal data from the Austrian Federal Welfare Office (FWO) from 1970–2003, which reports disability status, disability type, and disability degree for all individuals who are disabled in the context of the DPEA. One advantage of this type of information is that a medical procedure (rather than self-reported by firms or workers) objectively assesses the disability status. Note, however, that the FWO data set is inflow-based. This means that the stock of disabled workers might be incompletely captured in the early stages of this data. This drawback gradually vanishes if a snapshot of the stock of disabled workers is taken at a later time. Accordingly, we will only use data from very recent years. We restrict the years of data to the time period from 1996–2003 (see the next paragraph for details). The ASSD and FWO data can be linked on the basis of a person identifier. This allows us to calculate two crucial pieces of information accurately: the number of the non-disabled workers and the number of disabled workers each firm employs. The former variables determines whether a firm is required to hire a disabled worker and the latter represents how many disabled workers each firm actually employs. Hence we can precisely determine whether and the extent to which a firm complies with the employment quota. The FWO checks firms' compliance with the employment quota on the first day each month. We account for this administrative *modus operandi* by creating a data set with monthly reference dates, all of which correspond to the first day of each month.<sup>15</sup>

We restrict the empirical analysis to the time period from 1996–2003 to account for the fact that data on the disability status is only reliable for the most recent time period (see preceding paragraph). We divide this time period into three sub-periods of equal lengths (30 months), *across* which regulations of the DPEA were changed, but *within* which the regulations of the DPEA were unchanged (see section 3.2 for details). These time periods are defined as follows: (i) from July 1996 to December 1998, (ii) from January 1999 to June 2001, and (iii) from July

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<sup>15</sup>Note that firms cannot simply hire disabled workers for one day in order to fulfill the employment quota. A regulatory restriction of the DPEA rules out this behavior. It even turns out that disabled workers have on average a substantially higher tenure than non-disabled workers. The average (s.d.) number amounts to 10.3 (8.7) years for disabled and to 6.1 (6.9) years for non-disabled workers. The difference of 4.2 years is statistically significant at the 1%-level. This calculation is based on a sample of 2,000,000 Austrian workers on August 1, 2000.

2001 to December 2003. We focus on the time period from January 1999 to June 2001 (non-compliance tax of € 150; no bonus for over-compliance; probationary period of 3 months), but will also provide evidence for the preceding and subsequent time period (the former is characterized by a bonus for over-compliance and a probationary period of one month; the latter by a non-compliance tax of € 196 and a probationary period of six months). The choice of the time period in the middle allows us to directly assess how the effect of the DPEA on firms' demand for disabled workers varies with these policy changes. We further restrict the analysis to firms in the private sector – those likely to pursue a clear, profit maximizing objective. In particular, we look at firms operating in the services sector, manufacturing, construction, and the tourism industry.

### 3.4.2 Identification

Our empirical strategy is based on the fact that the DPEA discontinuously changes the financial incentives for employing disabled workers. The DPEA requires that firms hire a disabled worker if the size of the firm (as measured by the number of non-disabled workers)  $S_i$  is greater than or equal to the quota threshold  $T \in \{25, 50, 75, \dots\}$ . Firms that do not comply are subject to a non-compliance tax. This creates financial incentives for firms to hire disabled workers as firms face a trade-off between hiring a disabled worker or paying a compensation to be rid of this obligation.

Our aim is to identify the causal effect of the non-compliance quota tax on disabled employment. This causal parameter provides information on the effect of being subject to the tax at the factual threshold of 25, 50, etc. compared to moving the tax threshold up or down by a small unit. This parameter is important for policy. Recall that various OECD countries implement different quota. Thus, the threshold is a key element of employment quota policies. Our analysis aims to assess the role of setting the quota at 1 disabled worker in 25 non-disabled workers compared to setting it at 1 in 24 or 1 in 26 non-disabled workers.

Our empirical strategy contrast threshold firms' disabled employment to employment decisions expected from firms below the threshold to learn about the causal effect of the non-compliance tax on disabled labor demand. This identification strategy builds on the key behavioral assumptions that both disabled labor demand and supply are continuous in non-disabled employment at the threshold. Labor supply of disabled workers is clearly continuous in firm size because none of the DPEA provisions except the quota (increased employment protection, wage subsidies, workplace accommodation, etc.) change with firm size. Labor demand is continuous if firms do not endogenously choose their location with respect to the threshold. Section 3.3 argues that firms may manipulate the number of non-disabled workers but the extent of manipulation is low for low non-compliance taxes. We argue that this is rather the case in Austria. Recall that the tax amounted to 150 € in 1999, which is only

8 % relative to the average wage of Austrian employees or 0.32 % relative to the wage bill for 25 non-disabled workers. In section 3.4.4 we provide evidence that the non-manipulation assumption seems to hold. We are, however, aware that if the non-manipulation assumption fails to hold (since non-disabled employment is endogenous just as disabled employment if the tax is sufficiently high), our empirical strategy provides a *lower bound* on the the causal effect of non-compliance taxes rather than the causal effect.

### 3.4.3 Estimation

This sub-section provides an outline of the models we use to estimate the discontinuity in disabled employment at the quota threshold.

**Basic RD regression for quota threshold  $T = 25$**  The following linear regression allows identification of the discontinuity in the average number of disabled workers per firm at treatment assignment threshold  $T = 25$ :

$$Y_{it} = \alpha_0 + \alpha_1 \cdot D_{it} + \beta_0 \cdot \tilde{S}_{it} + \beta_1 \cdot D_{it} \cdot \tilde{S}_{it} + \epsilon_{it}, \quad (3.1)$$

where  $Y_{it}$  denotes the number of disabled workers,  $D_{it}$  indicates whether a firm is treated or not, and  $\tilde{S}_{it} = S_{it} - T \in [-12, 12]$  denotes the difference between current non-disabled employment  $S_{it}$  and threshold  $T = 25$  of firm  $i$  at date  $t$ . Including  $\tilde{S}_{it}$  is important since non-disabled employment will turn out to be strongly correlated with disabled employment.

The key parameter is  $\alpha_1$ . This parameter measures the average causal effect of DPEA on the number of disabled workers for firms at the quota threshold  $T$ .  $\alpha_0$  measures the average number of disabled workers for firms just below the assignment threshold  $T$ . The parameters  $\beta_0$  and  $\beta_1$  capture the correlation between firm size  $S_{it}$  and the average number of disabled workers per firm.

**Covariates** We will also use a ‘long’ version of model (3.1) that includes covariates measuring (i) firm size dynamics, (ii) characteristics of firms’ workforce, (iii) firms’ industry affiliation, and (iv) firms’ geographical location (at the state-level), and time fixed-effects. This model looks as follows.

$$\begin{aligned} Y_{it} = & \alpha_0 + \alpha_1 \cdot D_{it} + \beta_0 \cdot \tilde{S}_{it} + \beta_1 \cdot D_{it} \cdot \tilde{S}_{it} \\ & + X'_{it} \cdot \gamma_1 + X'_{it} \cdot \tilde{S}_{it} \cdot \gamma_2 + \pi_t + \theta \cdot \tilde{S}_{it} \cdot \pi_t + \epsilon_{it}, \end{aligned} \quad (3.2)$$

where  $X'_{it}$  is a vector that contains the full set of control variables (see table 3.2 for a detailed list of these control variables) and  $\pi_t$  are time fixed effects (a dummy for each reference month). In addition, model (3.2) includes an interaction between  $\tilde{S}_{it}$  and  $X_{it}$  as well as between  $\tilde{S}_{it}$  and  $\pi_t$ .  $X_{it}$  controls for firm characteristics and  $\pi_t$  controls for changes over time

that potentially affect the hiring strategy of either disabled or non-disabled workers, such as economic conditions, for example. The interactions between  $\tilde{S}_{it}$  and  $X_{it}$  and between  $\tilde{S}_{it}$  and  $\pi_t$  allow for different effects of the forcing variable  $\tilde{S}_{it}$  for different types of firms. Note, that the inclusion of all these covariates should not affect the estimated discontinuity, if the no-manipulation assumption holds.

**Functional form of trends in  $\tilde{S}$**  Note that the discrete support of the assignment variable  $\tilde{S}_{it}$  implies that we need to extrapolate in order to predict the counterfactual for threshold firms, i.e. we need to extrapolate the number of disabled workers threshold firms employ in the absence of the non-compliance tax.<sup>16</sup> Model (3.1) and (3.2) both assume a linear functional form. Mis-specification of this functional form would lead to a biased estimate of the discontinuity. Lee and Lemieux (2010) suggest two approaches to assess sensitivity to functional form. The first approach – polynomial approximation – adds higher order polynomials to the baseline model (3.1). The second approach – local linear regression – keeps the linear functional form but reduces the bandwidth. We implement both approaches to discuss sensitivity of the baseline model to functional form assumptions.

**Adjusted model for pooled quota thresholds  $T > 25$**  For the investigation of the pooled higher thresholds  $T > 25$ , we extend models (3.1) and (3.2) with a set of threshold dummies  $G_{it}$  that reflect the threshold that is closest to firm  $i$  at date  $t$  to control for differences in non-disabled employment across normalized thresholds (note that, for pooled quota thresholds  $T > 25$ ,  $\tilde{S}_{it}$  denotes the difference between current non-disabled employment  $S_{it}$  and threshold  $T_{it}$  that is closest to firm  $i$  at date  $t$ ).<sup>17</sup> In addition, to allow for effect heterogeneity across thresholds, we include interactions between the threshold dummies  $G_{it}$  and (i) the treatment indicator  $D_{it}$ , (ii) the normalized firm size  $\tilde{S}_{it}$ , and (iii) the interaction between  $D_{it}$  and  $\tilde{S}_{it}$ . Thus, the treatment effect  $\alpha_1$  in this model can be interpreted as an inverse variance weighted average of the threshold specific treatment effects (see Angrist, 1998).

### 3.4.4 Manipulation Checks

We now present results of the two key tests for manipulation of firm size suggested by Lee and Lemieux (2010). First, recall that the identification strategy is only valid if demand for

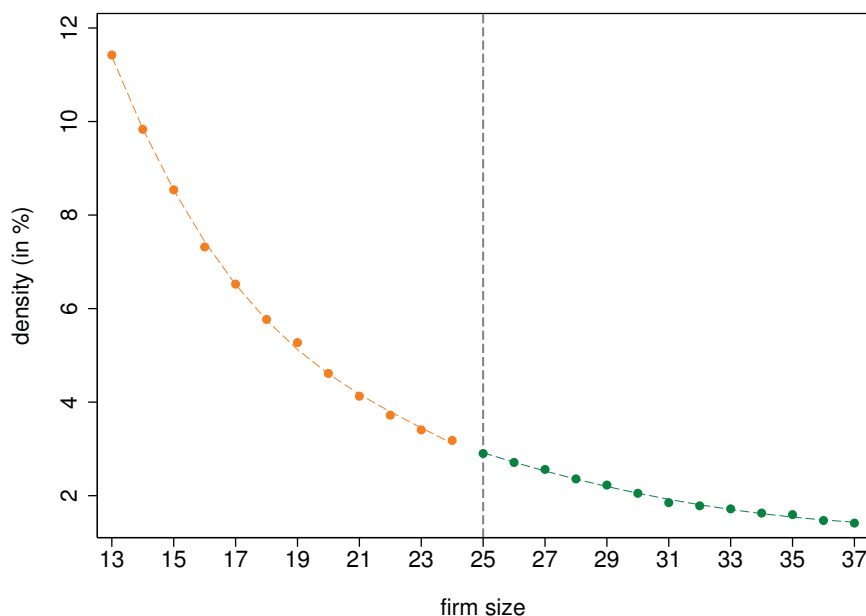
<sup>16</sup>Discrete support of the assignment variable also affects the variance-covariance matrix estimates. Lee and Card (2008) suggest using cluster-consistent standard errors (clustered on the distinct values of  $E_{it}$ ) to account for the uncertainty related to the choice of the functional form. Furthermore, remember that we use pooled cross-section data for the econometric analysis. Observations of the same firm cannot be considered to be independent from each other. Thus, we not only need to cluster on  $E_{it}$  but also on firms (note that this is non-nested). Cameron *et al.* (ming) propose a new variance estimator for OLS that provides cluster-robust inference when there is two-way clustering that is non-nested. As a consequence, we report two types of robust standard errors in our regression outputs: standard errors that are (i) clustered on  $E_{it}$  and (ii) those that are clustered on  $E_{it}$  and firms.

<sup>17</sup>Let  $G_{it} = \text{floor}((E_{it} + 12)/25)$  indicate a firm's threshold group, i.e.  $G_{it} = 1$  for firms located around the threshold at firm size 25,  $G_{it} = 2$  for firms located around the threshold at firm size 50, etc.



disabled workers is continuous in firm size. This assumption would be violated if firms self-select around the threshold. We assess whether there is endogenous selection of firms at the quota threshold studying firm size density. Firms might stay just below the threshold in order to avoid becoming subject to the non-compliance tax. If this endogenous sorting behavior related to the DPEA is present, we would expect a spike in the firm size distribution just below the threshold. Figure 3.2 reports the firm size distribution around the quota threshold  $T = 25$ . Visual inspection suggests that no important spike is present. We also formally test for the presence of a discontinuity in the firm size distribution (see McCrary, 2008). We apply model (3.1) with a cubic trend in  $\tilde{S}$  using the density of the firm size distribution (in %) as outcome variable. The parameter measuring possible discontinuities at the threshold is insignificant for the threshold  $T = 25$ .<sup>18</sup> Clearly, there is no spike in the firm size distribution.

Figure 3.2: Firm size distribution at quota threshold  $T = 25$



Notes: Discontinuity at threshold = 0.1464 with standard error = 0.1264 (adjusted for clustering on firm size), based on model 3.1 with a cubic trend in  $\tilde{S}$  using the density of the firm size distribution (in %) as outcome variable (number of observations = 25). Source: Own calculations, based on ASSD and FWO.

Second, our identification strategy only works if firms are identical at the threshold. While we can not test this assumption for unobserved characteristics, we provide evidence on continuity of means of observed background characteristics of firms. Table 3.2 reports key background statistics on firms located around the threshold  $T = 25$ . The first line provides information on firm size – the number of jobs provided to non-disabled workers – for firms above and below

<sup>18</sup>There is no discontinuity at pooled thresholds  $T > 25$  either. See supplementary results.

the threshold  $T = 25$ . We refer to firms employing 25 non-disabled workers or more as treated firms. These firms face the non-compliance tax if they do not provide a job to at least one non-disabled worker. We refer to firms below the threshold as control firms. These firms will be used to assess the counterfactual hiring decisions.

Table 3.2 indicates that treated firms differ from control firms. Treated firms are, by construction, larger than control firms. Whereas control firms employ 17.11 non-disabled workers on average (column 1), treated firms employ almost 30.14 non-disabled workers (column 2) almost twice as many as control firms. The difference in firm size is statistically significant at any conventional level (column 3).

Table 3.2 also displays information on firm size dynamics. The firm characteristic “employment stability” indicates whether the work force in month  $t$  was subject to any changes since month  $t - 1$ . The characteristic “expanded since 6 months” measures whether firm size in month  $t$  is strictly larger than firm size in month  $t - 6$ . The firm characteristic “contracted since 6 months” measures whether firm size in  $t$  is strictly smaller than in month  $t - 6$ . The workforce of treated firms is significantly less stable than control firms. Whereas 41 % of control firms have an unchanged workforce, this number only amounts to 26 % for treated firms. In terms of employment growth, results indicate that 47 % of treated firms and 45 % of control firms expanded during that past 6 months, the difference of 2 percentage points being statistically significant. In contrast, 39 % of treated firms downsized within the last six months compared to 34 % of control firms. This suggests that treated and control firms differ more strongly in terms firm size contractions than in terms of expansion of the non-disabled workforce.

To shed more light on how treated and control firms differ, Figure 3.3 plots mean employment stability (a), mean wage (b), and mean firm age (c) as a function of firm size. Figure 3.3 clearly indicates that employment stability is a strongly decreasing monotone function of firm size. Almost half of all firms employing 13 non-disabled workers saw their employment adjusted between this month and the previous one. In contrast, only about one in 5 firms with 37 jobs for non-disabled workers saw a change to their employment level. Most importantly, visual inspection suggests there is no strong difference in employment stability for firms with 24 or 25 non-disabled workers. Similar patterns are observed for wage and firm age.

Table 3.2 implements Model 3.1 for each of the background characteristics and presents the estimate of parameter  $\alpha_1$  in column 4.<sup>19</sup> Doing so formally tests whether the actual means for threshold firms differ from the means expected from control firms. Results are striking. The salient difference in employment stability from Table 3.2 column 3 disappears completely once we focus on threshold firms. This means that threshold firms and firms just below the threshold are identical in terms of employment dynamics. This result corroborates our earlier

<sup>19</sup>All p-values in column 4 of table 3.2 are adjusted for multiple testing according to Holm (1979).

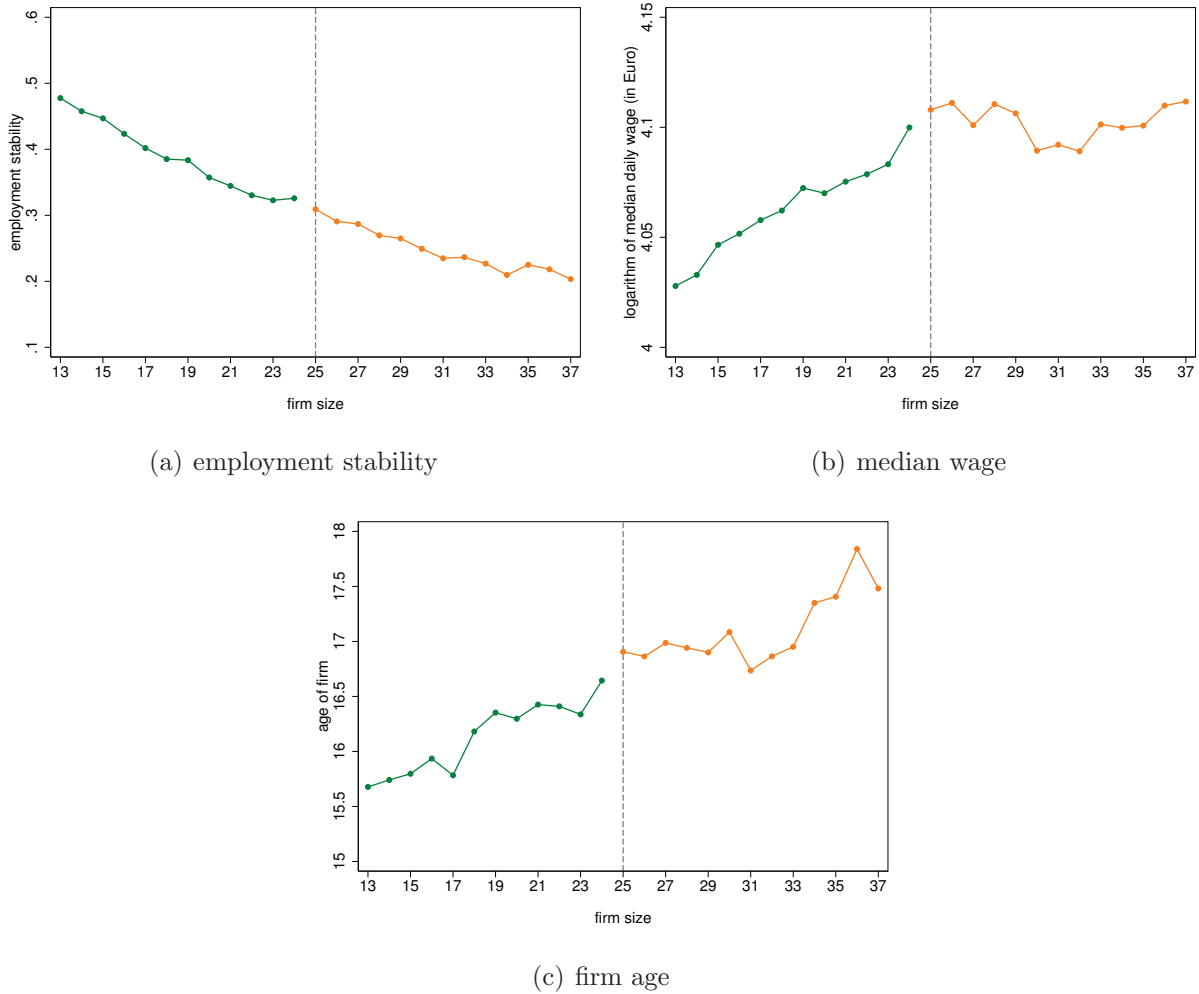


Table 3.2: Descriptive statistics around quota thresholds (time period: January 1999 - June 2001)

	below threshold		above threshold		difference	discontinuity <sup>b,c</sup>
	mean		mean			
Firm size	17.1139		30.1447		13.0308***	n/a
Firm size dynamics						
employment stability <sup>a</sup>	0.4092		0.2555		-0.1537***	0.0121
expanded since 6 months <sup>a</sup>	0.4473		0.4696		0.0223***	-0.0150
contracted since 6 months <sup>a</sup>	0.3408		0.3881		0.0473***	0.0038
Characteristics of firms' workforce						
logarithm of median daily wage <sup>a</sup> (in €)	4.0551		4.1028		0.0477***	0.0032
tenure <sup>a</sup> (in years)	5.3589		5.6305		0.2716***	0.0255
fraction women <sup>a</sup>	0.4083		0.3743		-0.0341***	0.0110
fraction white-collar <sup>a</sup>	0.4496		0.4458		-0.0038	0.0151
number of apprentices <sup>a</sup>	1.3625		2.0429		0.6804***	-0.0494
workers' age <sup>a</sup>	35.4687		35.7083		0.2396***	0.0376
age of firm (in years)	16.0121		17.0554		1.0433***	0.0793
Industry						
services	0.4556		0.4486		-0.0070	0.0107
manufacturing	0.2764		0.2935		0.0171**	-0.0091
construction	0.1676		0.1718		0.0043	-0.0045
tourism	0.1004		0.0861		-0.0143***	0.0030
Number of firm-month observations	330,427		117,729		448,156	448,156
Number of firms	22,311		9,058			
Total number of firms	25,755					

Notes: <sup>a</sup> Variable bases on characteristics of non-disabled workers only. <sup>b</sup> The estimated discontinuity is based on the following model:  $x_{it} = \alpha_0 + \alpha_1 \cdot D_{it} + \beta_0 \cdot \bar{S}_{it} + \beta_1 \cdot D_{it} \cdot \bar{S}_{it} + \epsilon_{it}$ , where the coefficient  $\alpha_1$  detects discontinuities in the mean of characteristics  $x_{it}$ . <sup>c</sup> The p-values for the estimated discontinuities are adjusted for multiple testing according to Holm (1979). \*\*\*, \*\*, \* denotes significance at the 1%, 5%, and 10% level respectively (standard errors are adjusted for clustering on firm size). Source: Own calculations, based on ASSD and FWO

Figure 3.3: Selected controls (mean) vs. firm size



Notes: Figure plots the mean of selected controls vs firm size. We selected three controls with means that vary strongly with firm size. The means of these selected controls but also of those we do not report are continuous in firm size at the threshold. The same result holds for pooled thresholds. See supplementary web appendix for detailed results. Source: Own Calculations, based on ASSD and FWO

finding regarding the absence of manipulation in the firm size distribution.

Table 3.2 provides further background information on firms. It reports information on the median wage paid to non-disabled workers, average tenure, the fraction of women, the fraction of white-collar workers, the average number of apprentices, workers' average age, and the average age of firms. Firm age measures the number of years the firm number has been observed in ASSD since 1972 – the year ASSD started.<sup>20</sup> Because the purpose of the empirical

<sup>20</sup>Note that this implies that firm age is left censored. Left censoring is not problematic in this application because the focus of this chapter is to measure the effects of the employment quota on employment of disabled workers. This means that information on firm age is merely used to control for differences between treated firms and control firms. Moreover, firm age will turn out to be balanced between threshold firms. This implies that left censoring of firm age is unlikely to bias estimates of the effect of employment quota on employment

analysis is to understand firm hirings of disabled workers, all firm characteristics are based on non-disabled workers employed by the firm in month  $t$ . Results indicate that control firms pay their employees about 4.06 (log) € per day, whereas treated firms pay almost 4.10 (log) € per day. This means that treated firms pay almost 4 percent more than control firms. With respect to tenure, table 3.2 shows that control firm employees have been working for their current employer on average for 5.4 years whereas treated workers have been with their employer slightly longer (5.6 years). There are also differences between treated and control firms with respect to the fraction of women (37 % vs. 41 %), and the number of apprentices (2.0 vs. 1.4), workers' age (35.7 vs. 35.5 years). In contrast, the fraction of white-collar workers is balanced. The average control firm was founded 16.0 years before the current date, whereas treated firms were established almost exactly 1 year earlier. There are also moderate differences in manufacturing and tourism firms.

Do these differences remain when moving to the threshold? Strikingly, results indicate that firms on either side of the  $T = 25$  threshold are perfectly balanced with respect to observed covariates (see column (4) of table 3.2). This means that all of the differences in covariates shown in column (3) of table 3.2 are not due to purposeful self-selection of firms but due to underlying differences in firm size. Finally, as we discussed above, we find no asymmetry in firm size density at the threshold. Overall, results in column 4 of table 3.2 indicate that firms on either side of the threshold are *observationally identical*. We therefore conclude that, despite the running variable non-disabled firm size being endogenous, our identification strategy boils down to a regression discontinuity design.

## 3.5 Econometric Results

This section discusses the causal effect of non-compliance taxes on disabled employment.

### 3.5.1 Results for Quota Threshold $T = 25$

This section presents the main econometric estimates of the effects of the DPEA on the number of disabled workers per firm at quota threshold  $T = 25$  (for the effect of the employment quota at thresholds higher than  $T = 25$  see further below).<sup>21</sup> Figure 3.4 reports the number of disabled workers per firm by firm size for sizes ranging from 13 to 37. The evidence is based on 448,156 firm-month observations, providing information on the employment decisions of 25,755

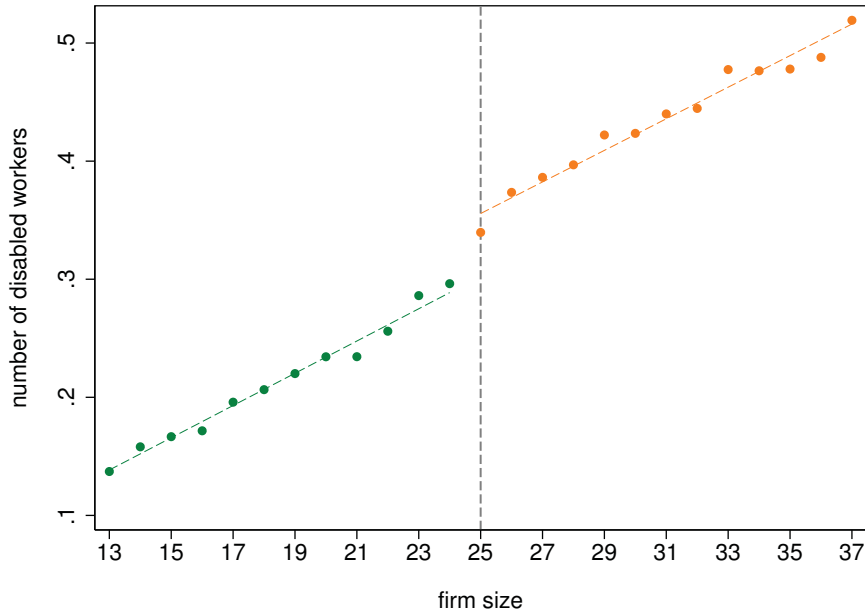
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of disabled workers.

<sup>21</sup>We put our main focus to this threshold for two reasons. First, firms at higher order thresholds are already subject to the quota system. Studying the first threshold allows analyzing the effects of being subject to or free of the quota system. Second, there are much fewer firms at higher order thresholds than at the first threshold. This means that the first threshold is the most relevant threshold in terms of the number of firms subject to the quota.

firms. Descriptive evidence indicates that the average number of disabled workers employed by firms below the quota threshold is lower than the number of disabled workers employed by firms subject to the quota. Specifically, firms that employ 13 non-disabled workers offer about 0.14 workplaces to disabled workers – 1 out of 7 firms provides employment to disabled workers. In contrast, firms that employ 24 non-disabled workers provide 0.30 jobs to disabled workers. Figure 3.4 also suggests an approximately linear increase in the mean number of jobs provided to disabled workers as firm size increases. Strikingly, quota threshold firms with 25 non-disabled workers appear to offer 0.34 jobs to disabled workers, an unexpected increase, given the behavior of firms not subject to the employment obligation. Again, the number of jobs provided to disabled workers increases in an almost linear fashion from firm size 25 to firm size 37.

Figure 3.4: The effect of the DPEA on the number of disabled workers at quota threshold  $T = 25$



Notes: Discontinuity at threshold = 0.0533 with standard error = 0.0075 (adjusted for clustering on firm size), based on model 3.1 (number of observations = 448,156). Source: Own calculations, based on ASSD and FWO.

Figure 3.4 thus presents evidence of an unexpected change in the average number of jobs provided to disabled individuals. This change can be measured by superimposing the fit of the model (3.1). Doing so yields a discontinuity at the quota threshold  $T = 25$  of 0.0533 (the standard error is adjusted for clustering on firm size is 0.0075; the standard error adjusted for clustering on firm is 0.0156; the standard error adjusted for clustering on firm size and firm is 0.0133). This discontinuity is statistically significant at the 1%-level. Prima facie, the effect appears to be quantitatively small. Note, however, that the mean number of disabled workers per firm around the first threshold is 0.26, meaning that the discontinuity constitutes a

20.9% increase in the number of disabled workers per firm ( $= 0.0533/0.2550$ ). Put differently, roughly one out of 20 firms employs one additional disabled worker due to the DPEA.

Table 3.3 shows our main results for the effect of the employment quota on jobs provided to disabled workers. Note first that the choice of clustering on the firm size, or on the firm size *and* firm, does not affect the statistical significance of our results in any of the 4 columns. Column (1) of table 3.3 shows results for model (3.1), thus simply repeating the results of figure 3.4. Column (2) shows results for model (3.2), i.e. with the full set of controls, time fixed-effects, and their respective interaction with firm size. The estimated discontinuity only slightly changes from 0.0533 to 0.0545. We conclude that the effect is very robust to the inclusion of covariates, which enforces the plausibility of our assumption that there is no manipulation in the number of non-disabled workers on that is associated with the presence of the non-compliance tax. Column (3) narrows the bandwidth to  $\tilde{S}_{it} \in [-6, 6]$ . The estimated discontinuity becomes smaller, amounting to 0.0373 with this smaller bandwidth. The inclusion of second order polynomials in  $\tilde{S}_{it}$  in column (4) to the specification in column (2) leads to the same effect as narrowing the bandwidth. The estimated discontinuity amounts to 0.0366 being almost identical to that of column (3). Thus, the baseline model (3.1) is sensitive to changes in functional form. The sensitivity analyzes in column (3) and (4) are, however, quite consistent regarding the causal effect of the non-compliance tax. We therefore adopt the model in column (3) as the baseline model for the remainder of the chapter. Note that results are not sensitive to adopting the model in column (4).

Table 3.3: The effect of the employment quota on the number of disabled workers per firm at quota threshold  $T = 25$  (time period: January 1999 - June 2001)

	Number of disabled workers			
	0.2550	0.2550	0.3081	0.2550
Mean	0.2550	0.2550	0.3081	0.2550
Standard deviation	0.6390	0.6390	0.7064	0.6390
Treatment effect	0.0533	0.0545	0.0373	0.0366
Cluster: $S$	(0.0075)***	(0.0081)***	(0.0075)***	(0.0084)***
Cluster: $S$ , firm	(0.0133)***	(0.0136)***	(0.0091)***	(0.0098)***
$S \in 25 \pm h$	$h = 12$	$h = 12$	$h = 6$	$h = 12$
Polynomial order in $(S - 25)$	1	1	1	2
Controls	No	Yes	Yes	Yes
Controls $\cdot (S - 25)$	No	Yes	Yes	Yes
Time fixed-effects	No	Yes	Yes	Yes
Time fixed-effects $\cdot (S - 25)$	No	Yes	Yes	Yes
Number of observations	448,156	448,156	183,678	448,156
$R^2$	0.0304	0.0697	0.0517	0.0698
Adjusted $R^2$	0.0304	0.0695	0.0512	0.0695

Notes: \*\*\*, \*\*, \* denotes significance at the 1%, 5%, and 10% level respectively. Robust standard errors in parentheses. Source: Own Calculations, based on ASSD and FWO

Is this effect quantitatively large? A lower bound on the extent to which firms substitute disabled workers and non-disabled workers can be calculated as follows. The estimate of column (3) of the treatment effect suggests that the quota leads to 0.0373 more disabled workers holding a job in threshold firms – an increase of about 12 % compared to the recognized disabled workforce of 0.31 disabled workers at the quota threshold. This change in disabled worker employment is triggered by a non-compliance tax which stands on the order of 8 % of the median non-disabled worker wage (€ 150 in fine per month relative to about € 1,850 in wages per month). The elasticity of substitution between disabled workers and non-disabled workers is therefore at least on the order of 1.60.<sup>22</sup> As Acemoglu and Angrist (2001) conjecture, disabled and non-disabled workers are quite strong substitutes.

### 3.5.2 Placebo Regressions

To further assess the validity of our RD setup, we estimated discontinuities in the number of disabled workers per firm at firm sizes where there should be no discontinuities. Figure 3.5 shows the estimated discontinuities (according to our baseline model) for firm sizes 7–35 (including the true threshold at firm size 25). The pattern is striking. There is a clear-cut peak at the true threshold, which already begins to grow at around firm size 21 and then flattens out beginning at firm size 26. Note that this is not surprising – given that there is a true discontinuity at firm size 25. The bandwidth of our baseline model is 6, thus the discontinuities calculated at ‘placebo’-thresholds 19–24 already consider treated firms.

The estimated discontinuities for the placebo thresholds at firm sizes 7–18 and 31–35, for which either only treated or only control firm are considered, are in all but five instances (at firm sizes 9, 10, 13, 14, and 34) statistically not different from zero at the 5%-level. Note, however, that we test a large number of coefficients at once. Therefore we need to adjust the p-values for the issue of multiple testing. It turns out that once we adjust the p-values for multiple testing according to Holm (1979), only the estimated discontinuity of the true threshold at firm size 25 remains statistically significant at the 5%-level.<sup>23</sup> This strongly

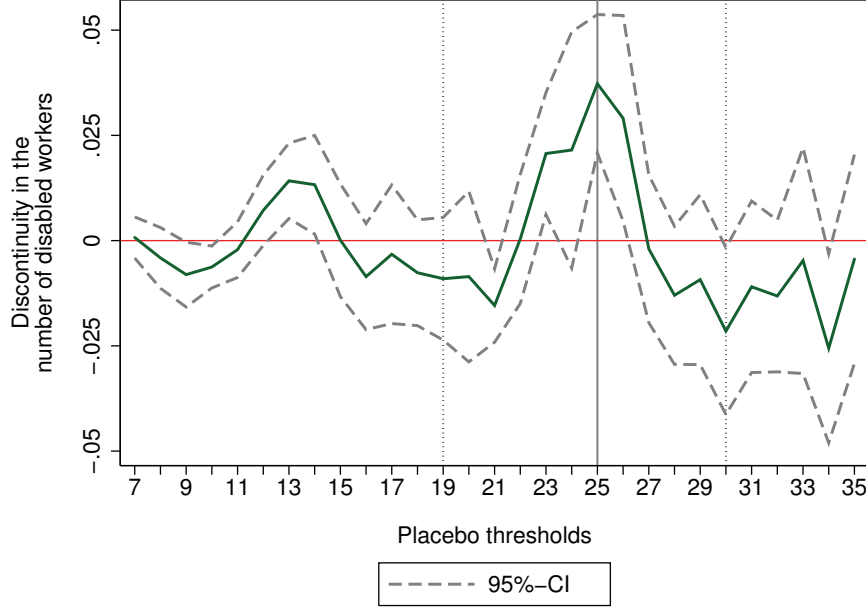
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<sup>22</sup>Recall that the elasticity of substitution is the negative of the percentage change disabled to non-disabled employment caused by a percentage change in the relative disabled to non-disabled wage. Consider the first threshold. Disabled to non-disabled employment increases by 0.15 % (effect of 0.0373 divided by threshold firm size of 25) because the tax reduced the relative wage of a disabled worker by about 7.5 percent (regular monthly earnings are 1,850 EUR; this means that the tax decreases disabled to non-disabled relative earnings from  $1,850 / 1,850$  to  $1,850 / 2,000$ ). The relative disabled and non-disabled wage is 1 whereas the disabled to non-disabled employment level stands at  $0.31/25$ . Thus the elasticity of substitution of threshold firms stands at about 1.60 ( $= -(0.0373/25)/(1850/2000 - 1) \cdot 1/(0.31/25)$ ). The corresponding elasticity for the threshold firm with 100 non-disabled workers is 1.58 ( $= -(0.0636/100)/(2000/2150 - 1) \cdot 1/((2.3044/4)/100)$ ). Note that average disabled employment is adjusted to reflect that the firm with 100 workers has already passed three thresholds and regular monthly earnings are 2,000 EUR rather than 1,850 EUR in large firms.

<sup>23</sup>We chose the Holm Method, which controls the *family-wise error rate* (FWE). As pointed out by Romano *et al.* (2008), this is the standard approach to account for multiple testing. Note that Romano *et al.* (2008) argue that this criterion can be too strict when the number of hypotheses under consideration is very large.

supports the credibility of our estimated discontinuity at firm size 25.

Figure 3.5: Testing continuity of mean disabled employment



Notes: Figure plots the parameter  $\alpha_1$  in regression  $y_{it} = \alpha_0 + \alpha_1 \cdot D_{it} + \beta_0 \cdot \tilde{S}_{it} + \beta_1 \cdot D_{it} \cdot \tilde{S}_{it} + \epsilon_{it}$  where  $y_{it}$  is number of disabled  $\tilde{S}_{it}$  is normalized firm size, i.e. firm size minus threshold, adopting a half-width  $h = 6$ . This parameter measures the difference in actual mean disabled employment at the threshold compared to mean disabled employment expected from data below the threshold for each threshold between firm size 7 to firm size 35. Note that all thresholds except threshold 25 are placebo thresholds. Parameter estimates within the vertical dashed lines can be affected by the factual discontinuity at firm size 25. Parameter estimates outside the vertical dashed lines can not be affected by the discontinuity. Source: Own Calculations, based on ASSD and FWO

### 3.5.3 Effects by low-wage and high-wage firms

Next, we turn to discussing heterogeneity of the treatment effect. Table 3.4 reports the causal effect of the employment quota for firms in different parts of the firm wage distribution at quota threshold  $T = 25$ . We group firms according to the median daily wage paid to their workers in the period 1999 to 2001. We then allocate each firm-month observation to four approximately equal sized groups based on the quartiles of the firm wage distribution. This grouping ensures that the relative size of the non-compliance tax decreases strongly. Whereas

However, in our context this does not apply (the number of hypotheses tested at once amounts to 29 in our context). We therefore stick to standard approach and do not apply the procedure of *false discovery proportion* (FDP) as suggested by Romano *et al.* (2008). The procedure of the Holm Method is as follows. The p-value of each estimated discontinuity is ranked from the smallest to the largest. The first p-value is multiplied by the number of investigated (placebo) threshold (29 in our case). The other p-values are consecutively – according to their rank – multiplied by the number of investigated (placebo) thresholds less the number of already adjusted p-values.



the average firm in the first quartile face a tax of 12.6 % of its firm wage, firms in the top quartile only face a tax of 5.6 % of the firm wage (bottom row in table 3.4).

Table 3.4: The effect of the employment quota on the number of disabled workers per firm by firms' median daily wage (quartiles) at quota threshold  $T = 25$  (time period: January 1999 - June 2001)

	Number of disabled workers			
	1 <sup>st</sup> Quartile	2 <sup>nd</sup> Quartile	3 <sup>rd</sup> Quartile	4 <sup>th</sup> Quartile
Mean	0.2868	0.3144	0.3033	0.3283
Standard deviation	0.6947	0.7148	0.6882	0.7261
Treatment effect	0.0758	0.0418	0.0261	0.0015
Cluster: $S$	(0.0241) <sup>***</sup>	(0.0185) <sup>**</sup>	(0.0104) <sup>**</sup>	(0.0076)
Cluster: $S$ , firm	(0.0261) <sup>***</sup>	(0.0216) <sup>*</sup>	(0.0148) <sup>*</sup>	(0.0095)
$S \in 25 \pm h$	$h = 6$	$h = 6$	$h = 6$	$h = 6$
Polynomial order in $(S - 25)$	1	1	1	1
Controls	Yes	Yes	Yes	Yes
Controls $\cdot (S - 25)$	Yes	Yes	Yes	Yes
Time fixed-effects	Yes	Yes	Yes	Yes
Time fixed-effects $\cdot (S - 25)$	Yes	Yes	Yes	Yes
Number of observations	47,575	46,519	43,818	45,766
R <sup>2</sup>	0.0606	0.0506	0.0642	0.0814
Adjusted R <sup>2</sup>	0.0585	0.0485	0.0620	0.0793
Tax as % of monthly wage	12.6%	9.0%	7.5%	5.6%

Notes: \*\*\*, \*\*, \* denotes significance at the 1%, 5%, and 10% level respectively. Robust standard errors in parentheses. Source: Own Calculations, based on ASSD and FWO

Results indicate that the employment quota produces a strong increase in the workplaces available to disabled workers among firms located in the first quartile of the wage distribution. Quota firms provide 0.0758 workplaces for disabled workers which would not be there without the employment quota (column 1). The estimated discontinuity in the number of disabled workers in the second quartile of the firm wage distribution is with 0.0418 much smaller (column 2). High-wage firms located above the median of the firm wage distribution respond considerably less than firms below the median. Threshold firms in the third quartile provide employment in excess of what would be expected from firms just below the threshold of 0.0261 workplaces (column 3) – or about a third the workplaces created by firms in the first quartile of the firm wage distribution. Interestingly, firms in the top quartile of the firm wage distribution do not appear to respond to the employment obligation (the estimated effect in column 4 is 0.0015). Note that the pattern of causal effects of the employment quota are very much in line with the pattern of relative impact generated by a flat rate tax.



### 3.5.4 Results for Pooled Quota Thresholds $T > 25$

This section investigates the effect of the employment quota for firms at the pooled higher quota threshold  $T > 25$ . The employment quota may act differently for large firms than for small firms. On one hand, large firms pay higher wages, implying that financial incentives should have less bite than for small firms. On the other hand, existing evidence strongly suggests that firm size is positively related to employment of the disabled. This may be because large firms find it easier to accommodate disabled workers. Table 3.5 shows the results for pooled higher thresholds ( $T = 50, 75, \dots$ ). Here we assign treatment status according to the deviation from thresholds  $\tilde{S}_{it} \equiv (S_{it} - T_{it})$ , where  $T_{it}$  represents the nearest threshold  $S_{it}$  is associated with. Firms are treated if  $\tilde{S}_i \geq 0$  and non-treated if  $\tilde{S}_i < 0$ . Remember from section 3.4.2 that all specifications in table (3.5) include threshold dummies  $G_{it}$ , as well as their interaction with the treatment indicator  $D_{it}$  and normalized firm size  $\tilde{S}_{it}$  and  $D_{it} \cdot \tilde{S}_{it}$ . Thus, the estimated discontinuity can be interpreted as an inverse variance weighted average of the threshold specific treatment effects. Column 1 shows that the effect amounts to 0.1387 if no additional covariates are included. The effect becomes smaller in column 2 if controls, time fixed-effects, and their respective interactions with the normalized firm size are added. The effect of having to offer a job to at least one additional disabled worker is 0.1071 – almost twice as large as the respective effect at quota threshold  $T = 25$ . Column 3 uses the smaller bandwidth. It turns out that the effect vanishes. If higher order polynomial are added instead, the effect becomes smaller (0.0775), but is statistically significant at least at the 10%-level if standard errors adjusted for clustering on the deviation from the threshold  $\tilde{E}$  are considered.

### 3.5.5 Effects by employment status before becoming disabled

This subsection provides separate estimates by the initial state before becoming recognized disabled. We decompose employment provided to disabled workers who had been employed with the same firm on the date of registration as disabled (*own former employees*), who had been employed with another firm on the date of registration as disabled (*other former employees*), and who had not been employed at the time of registration as disabled (*non-employees*).

Providing information on the effects for these three groups of workers is important to assess the likely mechanism that generates excess disabled employment at threshold firms. Threshold firms can retain their own former employees, poach employees from other firms, or create a new job for workers who were not employed when they acquired the recognized disabled status. Encouraging retention is clearly one of the main objectives of the DPEA (see section 3.2 on wage subsidies). Retention is also likely to conserve firm specific human capital more than generating excess employment through hiring from the non-employment pool or

Table 3.5: The effect of the employment quota on the number of disabled workers per firm at pooled quota thresholds  $T > 25$  (time period: January 1999 - June 2001)

	Number of disabled workers			
Mean	2.3044	2.3044	2.3833	2.3044
Standard deviation	5.3183	5.3183	5.4524	5.3183
Treatment effect	0.1387	0.1071	0.0636	0.0775
Cluster: $\tilde{S}$	(0.0504)**	(0.0394)**	(0.0425)	(0.0419)*
Cluster: $\tilde{S}$ , firm	(0.0580)**	(0.0524)**	(0.0490)	(0.0706)
$S \in T \pm h$	$h = 12$	$h = 12$	$h = 6$	$h = 12$
Polynomial order in $(S - T)$	1	1	1	2
Controls	No	Yes	Yes	Yes
Controls $\cdot (S - T)$	No	Yes	Yes	Yes
Time fixed-effects	No	Yes	Yes	Yes
Time fixed-effects $\cdot (S - T)$	No	Yes	Yes	Yes
Threshold dummies	Yes	Yes	Yes	Yes
Threshold dummies $\cdot D^a$	Yes	Yes	Yes	Yes
Threshold dummies $\cdot \tilde{S}$	Yes	Yes	Yes	Yes
Threshold dummies $\cdot \tilde{S} \cdot D^a$	Yes	Yes	Yes	Yes
Number of observations	220,187	220,187	111,746	220,187
R <sup>2</sup>	0.3859	0.4574	0.4651	0.4574
Adjusted R <sup>2</sup>	0.3854	0.4568	0.4639	0.4568

Notes: <sup>a</sup> The interaction term between the threshold dummies and the treatment indicator  $D$  is calculated with threshold dummies demeaned by  $E[\text{Threshold dummy}_j | D = 1]$ . \*\*\*, \*\*, \* denotes significance at the 1%, 5%, and 10% level respectively. Robust standard errors in parentheses. Source: Own Calculations, based on ASSD and FWO

from other firms.<sup>24</sup>

Table 3.6 provides information on the separate effects of DPEA on workers of different types. Column 1 in table 3.6 displays the baseline effect at the quota threshold  $T = 25$  (we repeat the estimate in column 3 of table 3.3 for ease of comparison). Results in column 2 suggest that quota threshold firms employ 0.0239 more disabled workers who had already been working for the firm before becoming recognized as disabled. This means that about 64 % of the baseline treatment effect at the quota threshold goes to workers who were already employed by their current employer. The resulting excess employment likely reflects the role of DPEA in increasing retention of existing employees. Whether the retention effect represents an increase in total employment is not clear. Firms may be relabeling existing workers. We think, however, that this retention effect implies an increase in employment of disabled individuals rather than just a relabeling of existing workers. First, the process of acquiring the status of a severely disabled worker is an involved process with a detailed medical assessment of

<sup>24</sup>Note, however, that these results do not speak about effects on total employment. Retained workers might have found work elsewhere, and workers who used to work at other firms may trigger new hiring at these other firms.

a workers work capacity. Hence relabeling a non-disabled worker as disabled is unlikely to happen. Second, since acquiring the disability status comes with substantial benefits to firms and workers, it is unlikely that an effectively disabled worker postpones acquiring the legal disability status to a date when the firm passes the quota threshold.

Table 3.6: Decomposing the treatment effect by employment status at date of registering as severely disabled at quota threshold  $T = 25$  (time period: January 1999 - June 2001)

	Number of disabled workers			
	baseline	own former employees	other former employees	non-employees
Mean	0.3081	0.1688	0.0940	0.0453
Standard deviation	0.7064	0.5069	0.3253	0.2377
Treatment effect	0.0373	0.0239	0.0127	0.0007
Cluster: $S$	(0.0075)***	(0.0050)***	(0.0018)***	(0.0022)
Cluster: $S$ , firm	(0.0091)***	(0.0055)***	(0.0026)***	(0.0031)
$S \in 25 \pm h$	$h = 6$	$h = 6$	$h = 6$	$h = 6$
Polynomial order in $(S - 25)$	1	1	1	1
Controls	Yes	Yes	Yes	Yes
Controls $\cdot (S - 25)$	Yes	Yes	Yes	Yes
Time fixed-effects	Yes	Yes	Yes	Yes
Time fixed-effects $\cdot (S - 25)$	Yes	Yes	Yes	Yes
Number of observations	183,678	183,678	183,678	183,678
R <sup>2</sup>	0.0517	0.0588	0.0328	0.0126
Adjusted R <sup>2</sup>	0.0512	0.0583	0.0323	0.0121
Percentage w.r.t. total effect	100	64	34	2

Notes: Own former employees are individuals who had been employed with same employer at date of registering as severely disabled. Other former employees are workers who had been employed with another employer at date of registering as severely disabled. Non-employees are workers who had not been employed at date of registering as severely disabled. \*\*\*, \*\*, \* denotes significance at the 1%, 5%, and 10% level respectively. Robust standard errors in parentheses. Source: Own Calculations, based on ASSD and FWO

Results in column 3 indicate that quota firms tend to have 0.0127 more employees on their payrolls who had been employed in different firms when becoming recognized as disabled. This means that up to 34 % of the treatment effect is generated by reallocating workers from other firms to firms at the quota threshold. Results in column 4 indicate that threshold firms' excess employment is not generated by increased hiring from the non-employment pool.

### 3.5.6 Policy Changes

So far, we have looked at the baseline period from January 1999 to June 2001 (period 2), when the non-compliance tax was roughly € 150, the probationary period amounted to three months and there was no bonus for over-compliance (see section 3.2 for details). Now we turn

to the preceding and subsequent time period, which differ in important ways with respect the regulations in place.

First, we turn to the time period from July 1996 to December 1998 (period 1). This period was characterized by the presence of a bonus for over-compliance. Firms that over-complied with the employment quota were awarded a bonus in the amount of € 52–76 for each excessive disabled worker and the probationary period for disabled workers was shorter than in our baseline period. The real value of the non-compliance tax for firms hiring at or above the non-disabled firm size of 25 remained unchanged. The column in the middle of 3.7 repeats for the sake of convenience the result for the baseline period for the quota threshold  $T = 25$ . Column (1) of table 3.7 shows the result for the preceding period where firms who provided a job to a disabled worker even if they did not have to received a bonus. The estimated discontinuity is with 0.0244 only two-third as big as in the baseline period. The bonus for over-compliance creates incentives for firms that are below the quota threshold to hire a disabled instead of a non-disabled worker, thus increasing the number of disabled workers hired in the absence of the non-compliance tax. In contrast, firms that are above the quota threshold are unaffected by the bonus in the sense that they need to hire a disabled worker in order to avoid becoming subject to the tax. This shift leads to a smaller discontinuity in the number of disabled workers at threshold  $T = 25$ .

Second, we turn to the time period from July 2001 to December 2003 (period 3). This time period is characterized by a higher non-compliance tax (and a longer probationary period) as compared to the baseline period. Column (3) of table 3.7 reports the result. The estimated discontinuity is substantially larger than that of the baseline period. A higher tax generates stronger incentives for firms to hire a disabled instead of a non-disabled worker.

Column 4 pools the entire time period from July 1996 to December 2003 and with full interactions with a dummy for period 1 and period 3. Results indicate that the difference between period 2 and 3 is statistically significant at least at the 10%-level (irrespective of the type of standard error). Note that this finding is also in line with Wuellrich (2010) (see chapter 4) who finds that the increase in the non-compliance tax had a positive impact on firms' demand for disabled workers. In contrast, the difference between the effect of the non-compliance tax in the period without bonus (column 2) and the period with bonus (column 1) is not significant if simultaneous clustering on firm and firm size is performed.

### 3.5.7 Wage Determination for Disabled Workers

The existing evidence suggests that threshold firms do react to financial incentives. A key element shaping demand for disabled workers is the wage paid to disabled workers. The DPEA states that disabled workers must be offered the same contract as non-disabled workers. This implies that firms can not set wages for job entrants differently for disabled and non-disabled

workers. But firms may affect wage growth within firms through promotion decisions. This section therefore provides an in-depth analysis of the determinants of the wages of disabled workers.

The analysis is based on 866,705 male workers (16,422 disabled and 850,283 non-disabled workers) on August 1, 2000.<sup>25</sup> We split this sample into white- and blue-collar workers. We regress the logarithm of the daily wage (in €) on a dummy for being disabled and tenure holding constant work experience (and its square), schooling (and its square), age (and its square), firm location, and industry.

Panel A of table 3.8 displays the results for male white-collar workers. Column (1) shows the baseline result. Disabled workers earn 5.4 % less than non-disabled workers. Column (2) fully interacts the dummy for being disabled with all covariates. The wage differential between disabled and non-disabled workers becomes larger amounting to 14.8 %. Moreover, column (2) shows that disabled workers are significantly less remunerated for each year of tenure (this differential persists until a tenure of 12 years). This suggests that disabled workers either are less promoted or sort into jobs with worse prospects. Column (3) sheds light on the starting wages by only considering workers with tenure of less than 3 months. The coefficient of the dummy for being disabled is not statistically different from zero indicating that disabled workers receive the same starting wage as non-disabled workers. Column (4) and (5) show results for a tenure of three months to five years and for more than five years. Consistent with the pattern of existing results, the wage differential becomes larger the more years of tenure a worker has accumulated.

Panel B of table 3.8 reports the results for male blue-collar workers. The baseline effect in column (1) amounts to -11.0 %, a magnitude almost twice as large as shown in column (1) of Panel A. This is a plausible finding if we account for the fact that jobs of blue-collar workers are physically more demanding. It is therefore more difficult for disabled workers to conduct the tasks as well as non-disabled workers for these kind of jobs. Column (2) furthermore shows that years of tenure are less compensated for disabled workers. Again, disabled workers seem to be less promoted than non-disabled workers. In contrast to panel (A), column (4) of panel B reveals that there are significant differences in the starting wage between disabled and non-disabled workers. Disabled workers are offered a starting wage that is 12.0 % lower than that of non-disabled workers. The difference remains large also for individuals with more years of tenure (see column (4) and (5)).

For white-collar workers, we find that entry-wages of disabled workers do not differ significantly from those of the non-disabled. Yet wage progression within firms appears to be different for disabled workers than non-disabled workers – a pattern of results that is consistent

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<sup>25</sup>We focus on men because the data do not contain information on hours and men typically tend to work full time.

with firms aiming to undo the productivity disadvantage of disabled workers over time. The situation is somewhat different for disabled blue-collar workers whose wage-tenure profile is significantly below those of their non-disabled co-workers already at when they start at their current firm.

## 3.6 Conclusion

This chapter analyzes the effect of an employment quota in promoting employment for disabled workers. While there is a considerable literature on the effects of anti-discrimination legislation, convincing causal evidence of employment quota systems is almost non-existent. Our study makes a first attempt to understand the role of employment quota for disabled workers in shaping the marginal firm's demand for disabled employment. This analysis complements existing evidence on anti-discrimination legislation.

The identification strategy relies on the sharp discontinuity in the relative costs of employing disabled workers created in a quota system combined with taxes raised on firms that do not comply with legal employment requirements. This design is a priori not a valid regression discontinuity design because firms may decide not to hire a non-disabled worker to avoid being subject to the tax. We discuss this issue in a simple behavioral framework and find that firms may manipulate threshold location but to only to a weak extent, and manipulation leads to a downward bias on the causal effect of non-compliance taxation. Moreover, our extensive set of manipulation checks finds no evidence for manipulation of the non-disabled workforce due to the employment quota. Thus we argue that applying a regression discontinuity design is a valid identification strategy in this study.

Results indicate that the quota promotes the employment of disabled workers in firms located at the quota threshold compared to the situation where the quota were increased by one worker. The quota leads to excess employment of 0.04 or loosely speaking one disabled worker per 25 threshold firms. We also detect important interactions between wages and firm size. Firms in the lower tail of the firm wage distribution tend to provide most of the excess employment to disabled workers. The employment quota leads to twice as much excess employment among large firms rather than among small firms (this effect is imprecisely estimated). We also find that the quota boosts employment primarily among former employees of the firm. The quota also encourages firms to poach workers from other firms but not individuals who were not formerly employed.

We conclude that the financial sanctions accompanying the employment quota do indeed increase compliance with the quota. This is a first result that is necessary for the quota to promote overall employment for disabled workers. We also show that the quota employment effect is not entirely due to reallocation of disabled workers between firms. Taken together,

these results suggest that overall disabled employment may increase due to the employment quota. However, the employment quota may also displace non-disabled workers leading to ambiguous effects on overall employment. Further research should therefore put emphasis on evaluating this policy instrument in other contexts and compare the relative effectiveness of quota with anti-discrimination legislation.

## Acknowledgments

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Table 3.7: The effect of the employment quota on the number of disabled workers per firm for different time periods

	Number of disabled workers			
	July 1996 – December 1998	January 1999 – June 2001	July 2001 – December 2003	July 1996 – December 2003
Mean	0.2735	0.3081	0.3373	0.3070
Standard deviation	0.6435	0.7064	0.7701	0.7105
Treatment effect	0.0244	0.0373	0.0607	0.0373
Cluster: $S$	(0.0054)***	(0.0075)***	(0.0094)***	(0.0075)***
Cluster: $S$ , firm	(0.0061)***	(0.0091)***	(0.0103)***	(0.0091)***
Treatment effect · Period 1				−0.0129
Cluster: $S$				(0.0072)*
Cluster: $S$ , firm				(0.0106)
Treatment effect · Period 3				0.0234
Cluster: $S$				(0.0105)**
Cluster: $S$ , firm				(0.0139)*
$S \in 25 \pm h$	$h = 6$	$h = 6$	$h = 6$	$h = 6$
Polynomial order in $(S - 25)$	1	1	1	1
Controls	Yes	Yes	Yes	Yes
Controls · $(S - 25)$	Yes	Yes	Yes	Yes
Time fixed-effects	Yes	Yes	Yes	Yes
Time fixed-effects · $(S - 25)$	Yes	Yes	Yes	Yes
Number of observations	179,968	183,678	192,953	556,599
R <sup>2</sup>	0.0500	0.0517	0.0465	0.0504
Adjusted R <sup>2</sup>	0.0495	0.0512	0.0460	0.0499
Amount of non-compliance tax (in €)	142 – 146	148 – 150	196	–
Bonus for over-complying (in €)	52 – 76	n/a	n/a	–
Probationary period (in months)	1	3	6	–

Notes: \*\*\*, \*\*, \* denotes significance at the 1%, 5%, and 10% level respectively. Robust standard errors in parentheses. Source: Own Calculations, based on ASSD and FWO



Table 3.8: Wages for disabled workers (men only; reference date: 08.2000)

	logarithm of daily wage (in €)				
	baseline	fully interacted	tenure < .25 years	tenure ∈ [.25, 5) years	tenure ≥ 5 years
<b>Panel A: White-collar workers (men only)</b>					
Mean	4.4871	4.4871	4.2529	4.4042	4.5534
Standard deviation	0.3966	0.3966	0.5416	0.4554	0.3228
Disabled worker	−0.0541*** (0.0047)	−0.1476*** (0.0229)	−0.0390 (0.0348)	−0.0374*** (0.0091)	−0.0592*** (0.0049)
Tenure (in 10 years)	0.1648*** (0.0068)	0.1653*** (0.0067)			
Tenure <sup>2</sup> (in 10 years)	−0.0405*** (0.0022)	−0.0406*** (0.0022)			
Disabled worker · tenure		−0.0347*** (0.0058)			
Disabled worker · tenure <sup>2</sup>		0.0149*** (0.0057)			
Number of Obs.	378,516	378,516	15,788	163,195	242,228
R <sup>2</sup>	0.1709	0.1717	0.1248	0.1233	0.1419
Adjusted R <sup>2</sup>	0.1709	0.1716	0.1238	0.1232	0.1418
<b>Panel B: Blue-collar workers (men only)</b>					
Mean	4.1658	4.1658	3.9502	4.0887	4.2372
Standard deviation	0.3878	0.3878	0.4587	0.4151	0.3392
Disabled worker	−0.1099*** (0.0086)	−0.2857*** (0.0345)	−0.1199*** (0.0272)	−0.1400*** (0.0113)	−0.1036*** (0.0101)
Tenure (in 10 years)	0.1505*** (0.0124)	0.1499*** (0.0119)			
Tenure <sup>2</sup> (in 10 years)	−0.0414*** (0.0051)	−0.0413*** (0.0049)			
Disabled worker · tenure		−0.0220** (0.0094)			
Disabled worker · tenure <sup>2</sup>		0.0181** (0.0089)			
Number of Obs.	477,536	477,536	30,162	201,402	300,822
R <sup>2</sup>	0.2053	0.2071	0.1327	0.1734	0.1503
Adjusted R <sup>2</sup>	0.2052	0.2070	0.1322	0.1733	0.1503

Notes: The interaction terms between tenure, experience, age, and schooling with the dummy for being a disabled worker is calculated with their respective values demeaned by  $E[\text{tenure, experience, schooling, age} \mid \text{disabled worker} = 1]$  in column 2. All regressions control for schooling, experience, age, firm location, and industry. Sample used for this analysis: male workers (reference date: 08.2000). \*\*\*, \*\*, \* denotes significance at the 1%, 5%, and 10% level respectively. Robust standard errors in parentheses (adjusted for clustering on firm). Source: Own Calculations, based on ASSD and FWO

## CHAPTER 4

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### The Effects of Increasing Financial Incentives for Firms to Promote Employment of Disabled Workers

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#### 4.1 Introduction

Policies that promote the employment of severely disabled individuals rank high on the policy agenda in many countries. The reason for that is that the labor market prospects of severely disabled individuals is highly unfavorable and the size of this particular group is at the same time far from being of minor importance (for more details see OECD, 2003). In one-third of all OECD countries, such as Austria, Belgium, France, Germany, Italy, Korea, Poland, and Spain, policy is based on a mandatory employment quota. According to such regulations, employers are obliged to have a certain proportion of disabled individuals among their staff, ranging from 2% in Spain to 7% in Italy. In case of non-compliance, employers are usually due to pay a tax per month for each place not filled, ranging from 0.25–4% of the monthly pay-roll of firms. However, the compliance rate only amounts to 50%, ranging from 25% in Spain to 67% in France. The main issue in current political discussion concerning such employment quotas is whether the tax is simply too low to provide sufficient financial incentives for firms to hire disabled workers. Not surprisingly, there is a strong call for increasing the tax.

This study investigates whether the unique tax increase from € 150 to € 196 in July 2001 in the context of the Austrian employment quota promoted the employment of disabled workers. I propose to identify the causal effect of this 30% tax increase on firms' demand for disabled

workers using the interrupted time-series approach (assessing the immediate as well as the short-run impact). This approach is appealing and in the present context superior to the difference-in-difference approach since a valid control group is unavailable. One particularly attractive feature of this study is very large and comprehensive data from the Austrian Social Security Database (ASSD) and the Austrian Federal Welfare Office (FWO). The combination of these two data sets allows me to determine the number of disabled and non-disabled workers each Austrian firm employs over time.

There are only very few economic studies that evaluate the effects of employment quotas for disabled workers on their employment.<sup>1</sup> Lalive *et al.* (2009) provide recent evidence on the effect of the Austrian employment quota shortly before the tax increase in Austria. They find that one out of twenty firms employs one disabled worker more than it would without the employment quota.

The remainder of the article is organized as follows. Section 4.2 provides an overview on the institutional situation in Austria. Section 4.3 describes the data. Section 4.4 outlines the empirical strategy and section 4.5 presents the results and their discussion.

## 4.2 Background

The employment quota in Austria is the main element of the Disabled Persons Employment Act (DPEA), which constitutes the most important instrument in the Austrian legal system to enhance the labor market opportunities of severely disabled individuals. It obliges firms to hire one disabled per 25 non-disabled workers. Firms that do not comply with this obligation are subject to a tax of currently € 213 per month and non-hired disabled worker (i.e. the tax acts as an implicit tax on hiring a non-disabled worker if a disabled worker would be required by the DPEA).<sup>2</sup> About two-third of all Austrian firms fully comply with this requirement. The tax was steadily increased from € 118 in 1990 to € 150 in June 2001 according to a inflation-based measure. On July 1, 2001, however, there was a unique and considerable increase in the amount of € 46 to € 196. Henceforth, it was again gradually increased. This 30% tax

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<sup>1</sup>Most previous studies on employment of disabled workers relate to general anti-discrimination legislation. For the U.S. context see e.g. DeLeire (2000), Acemoglu and Angrist (2001), Beegle and Stock (2003), Kruse and Schur (2003), Jolls and Prescott (2004), Jolls (2004), for the U.K. context see e.g. Bell and Heitmueller (2009), and for Germany's anti-discrimination legislation see e.g. Lechner and Vazquez-Alvarez (2009) and Verick (2004).

<sup>2</sup>The DPEA also defines how the revenues collected through non-compliance taxes are to be spent. The main beneficiaries are firms (and their disabled employees) who actually offer employment to disabled workers. These subsidies, either in form of allowances or loans, support those firms which employ at least one disabled worker. In particular, they are granted for adequate workplace accommodation, wage subsidies, work assistance, occupational retraining, or professional development. Basically, this represents a reallocation of resources from firms that fail to comply with the quota rule to firms that employ at least one disabled worker in order to compensate the latter for their effort in employing disabled workers.

increase amounts to roughly 1.5% of workers' average monthly salary or 0.19% of firms' average monthly pay-roll in the Austrian private sector in 2006.<sup>3</sup> The number of disabled individuals counting for the fulfillment of the quota is non-negligible. It amounted to over 91'000 in 2005 (roughly 2% of the total workforce in Austria).<sup>4</sup>

### 4.3 Data

To assess the impact of the tax increase, I use register data from two different sources: (i) the Austrian Social security database (ASSD)<sup>5</sup>, which contains detailed information on individuals' employment history since 1972 together with an unambiguous firm identifier, and (ii) register data from the Austrian Federal Welfare Office (FWO), which records the disability status of all disabled individuals. Linking these two data sets allows the accurate calculation of the number of disabled and non-disabled workers each firm employs. I create a data set with monthly reference dates from January 1999 to December 2002. I further concentrate on purely private sector firms. Finally, only firms with a firm size between 5–249 are kept (note that only 0.3 percent of all firms have firm sizes of 250 and above).<sup>6</sup> This sample consists of 2,879,025 firm-month observations (104,780 firms). For the main analysis, I use only firms that are subject to the non-compliance tax (i.e. firms with 25 or more non-disabled workers). This restricted sample consists of 500,439 firm-month observations (17,017 firms).

### 4.4 Empirical Strategy

I adopt the interrupted time-series approach (ITSA) (see Cook and Campbell, 1979) to identify the average treatment effect of the tax increase on the number of disabled workers per firm. This approach is appealing in the present context since there is no valid control group available.<sup>7</sup> The problem with the commonly used before-after estimator is that potential out-

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<sup>3</sup>Source: Statistics Austria

<sup>4</sup>The legal status of being disabled is extremely restrictive in the context of the DPEA. The disabled is approved only if a medical expert of the FWO assesses a degree of physical, mental, intellectual or sensuous disorder, which reduces work capacity by at least 50 percent.

<sup>5</sup>See Zweimüller *et al.* (2009) for a detailed description of the ASSD data.

<sup>6</sup>For firms with less than 5 employees, employees based firm characteristics are not well defined and are therefore excluded from the sample.

<sup>7</sup>For example, a difference-in-difference approach requires a valid control group. The two obvious candidates for serving as a control group are firms not covered by the DPEA and non-disabled workers. However, both control groups cannot be used. The former control group is not applicable since the conventional DID estimator requires that in absence of the treatment, the average outcomes for treated and controls would have followed parallel paths over time. This is implausible in our context as non-treated firms are not obliged to hire any disabled workers whereas treated firms are, independently of the tax increase. In addition, this issue cannot be overcome by the recent semi-parametric approach suggested by Abadie (2005) due to the common support problem regarding the firm size. The latter control group (non-disabled workers) is not applicable since, in the

comes may change with time, which the ITSA tries to overcome. The idea behind the ITSA is that past outcomes are the best predictor of future outcomes if the policy change had not taken place. This prediction allows to calculate the counterfactual number of disabled workers at the date of the tax increase. The crucial identifying assumptions are that (i) the outcome variable is continuous in time in the absence of the tax increase, and (ii) the treatment status is not related to the policy change. The first assumption is plausible since no further policy changes referring solely to firms with 25 and more non-disabled workers took place in July 2001, which may have affected the outcome variable. The second assumption needs a closer look. Firms are free to choose their firm size and hence their treatment status. The identifying assumption fails to hold if firms that would have chosen to employ at least 25 non-disabled workers without tax increase, did not hire as many workers due to the higher tax in place. In that case, the average treatment effect is upward biased. Whether or not this assumption holds is fundamentally untestable. However, I argue that it is only sensible for firms, which are close to the threshold of 25 non-disabled workers. It is rather unlikely that firms considerably adjust their firm size in order to avoid being subject to the tax. Moreover, in the presence of any seasonality the ITSA is sensible to the distribution of calendar month before and after the policy change. Therefore, I adjust the number of disabled workers per firm for seasonality. Moreover, I allow the time trend to differ before and after the tax increase. The idea behind this is as follows. The interruption in the time-series captures the immediate response of firms to the increased financial incentives to hire disabled workers. However, the overall effect may be bigger than this interruption actually reveals if some firms sluggishly respond to the tax increase (e.g. firms need time to search and/or to provide workplace accommodation to fill a job vacancy with a disabled worker, firms may only be able to learn over time whether they have only temporarily or permanently crossed the quota threshold, or excess employment may also build up over time due to retention). The short-run effect of the tax increase is captured by the difference in the time trend before and after the tax increase.

The following linear regression allows to identify the average treatment effect of the tax increase on the number of disabled workers per firm:

$$Y_i = \beta_0 + \beta_1 \cdot \text{After}_i + \beta_2 \cdot t_i + \beta_3 \cdot \text{After}_i \cdot t_i + FSD'_i \cdot \theta + X'_i \cdot \alpha + \epsilon_i,$$

where  $Y_i$  denotes the number of disabled workers (adjusted for seasonality).  $\text{After}_i$  indicates whether a firm is observed before or after the policy change.  $t_i \in \{-30, \dots, 0, \dots, 17\}$  captures the time trend (in the baseline specification I assume a linear time trend, but it is extended by

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context of the DPEA, firm size is defined as the number of non-disabled workers. Yet, this is an important firm characteristic by itself that confounds the number of disabled workers per firm (i.e. there is a strong positive relationship between non-disabled and disabled workers per firm) and at the same time determines whether a firm is subject to non-compliance taxation. Thus, non-disabled workers cannot be used as control group in this context either.

quadratic terms for robustness checks).  $FSD'_i$  is a vector of firm size dummies and  $X'_i$  includes control variables such as firm age, firm location, firm's industry affiliation, the number of non-disabled apprentices, characteristics of firm's average non-disabled workers (age, share of white-collar workers, share of women), and the median log daily wage paid to its non-disabled employees. The parameters of main interest are  $\beta_1$ , which measures the immediate response of the tax increase on the number of disabled workers, and  $\beta_3$ , which measures the short-run response to the tax increase, i.e. the change in the time trend after the tax increase.

## 4.5 Results and Discussion

I begin with providing some descriptive evidence. Figure 4.1 displays firms' average number of disabled workers (adjusted for seasonality,  $FSD_i$ , and  $X_i$ ) and superimposes a linear time trend (dashed lines) before and after the tax increase (the dashed vertical line denotes the date of the tax increase). We see that there is an interruption in the time trend at the date of the tax increase. Moreover, we see that whereas the slope of the time trend is literally zero before the tax increase, it has an strong upward trend after the tax increase. These two features provide strong evidence that the 30%-increase in the non-compliance tax had an immediate as well as short-run impact on firms' demand for disabled workers.

Figure 4.1: The number of disabled workers (adjusted for seasonality,  $FSD_i$ , and  $X_i$ ) over time

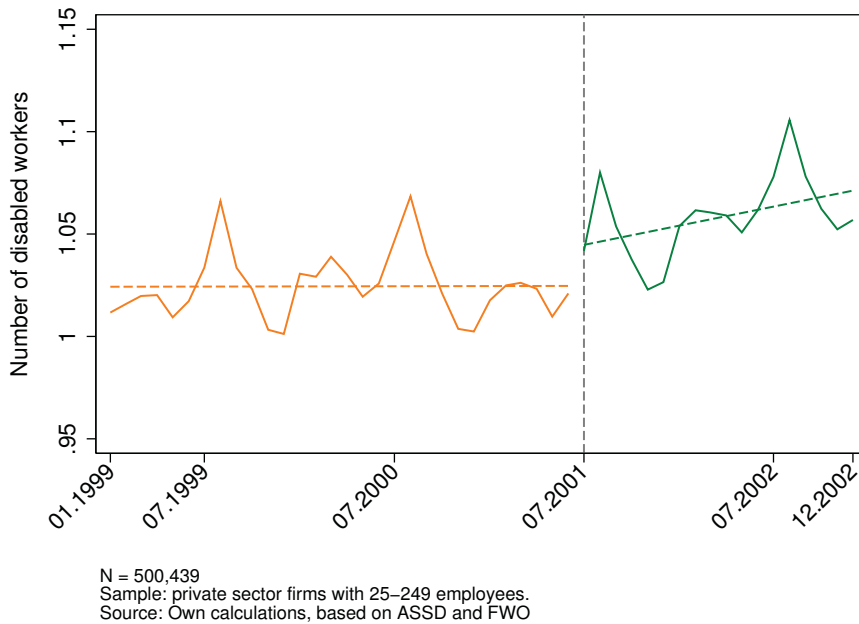


Table 4.1 shows the econometric results.<sup>8</sup> First, I discuss the immediate response to the tax increase. Column (1) displays the result for the linear fit in  $t_i$ . The immediate response amounts to 0.0202 and is statistically significant at the 1%-level. This means that firms employ 0.0202 disabled workers more than they would in the absence of the tax increase, which is in terms of the average number of disabled workers a 1.9% increase. Put differently, roughly one in 50 firms employs one disabled worker more due to tax increase. Adding quadratic terms in  $t_i$  does not change the results much. It only makes the effect with 0.0323 more pronounced (column (2)). Column (3) performs the same analysis as column (1), but keeps only firms with at least 31 non-disabled workers (see discussion on endogeneity of firm size in section 4.4). It turns out that the effect does not alter as compared to column (1). Controlling for firm fixed effects in columns (5)–(7) does not change the results with respect to the immediate responses. With the linear specification of the time trend (column (5)), it amounts to 0.0255 and increases to 0.0367 when adding quadratic terms in  $t_i$  (column (6)). Column (7) shows that the result is again very robust to the exclusion of the firms with 25–30 non-disabled workers.

Second, I look at the short-run responses of firms to the tax increase. It turns out that the time trend significantly changes after the tax had been increased (except for column (1)). The slope of the linear time trend increases by 0.0023 (column (5)). This means that roughly one in about 450 firms decide to employ one disabled worker more each month as a response to the tax increase, i.e. they indeed sluggishly respond to the tax increase. In column (6) – with a quadratic time trend – the slope in the time trend increases by 0.0035 in the first month and exhibits no statistically different decrease over time. Again, the results are robust to the exclusion of firms with 25–30 non-disabled workers. Columns (5)–(7) provide strong evidence that the time trend changes after the tax increase. Columns (1)–(3) (without firm

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<sup>8</sup>In order to assess the validity of the empirical design and the robustness of the results, I performed two additional analyses. First, I run several placebo regressions. The procedure is as follows. I restrict the sample to the time before the tax increase (January 1999 – June 2001), a sample that contains 311,480 firm-month observations. Then I run nine placebo regressions (linear time trend) using pseudo dates for the tax increase, which are in the center of this time-frame (December 1999 – August 2000). This choice ensures that I have a sound number of firm-month observations on either side of these pseudo dates. The results are as follows. Only one coefficient is statistically different from zero, but with a magnitude that is a third smaller than the estimate in column (1) of Table 4.1 (0.01324 vs. 0.02017) and with a p-value 5 times as large (0.034 vs. 0.007). None of the remaining 8 coefficients is statistically significant at the 5%-level (though four of them are marginally significant at the 10%-level) and all of them are considerably smaller than the estimate at the true date. The fact that the interruption in the time-series for the true date is larger than for any of the placebo dates (December 1999 – August 2000) and that its p-value is by far the smallest supports the plausibility of the empirical design. Second, I also checked whether the results are sensitive to the choice of the dependent variable. Instead of using the number of disabled workers per firm, I could also have defined the dependent variable as the percentage of disabled relative to non-disabled workers in each firm. The results do neither qualitatively nor quantitatively change much. This sensitivity check suggests that it is unlikely that the results are driven by a misspecification in the relationship between the dependent variable and the time trend since there is no obvious reason why the results should be similar irrespectively of the choice of the dependent variable using the same specification.



fixed-effects) show similar results.

It is of clear policy importance (as an anonymous referee suggested) whether the positive effect of the tax increase for firms being subject to non-compliance taxation is offset by a decrease in the number of disabled workers in firms not covered by the DPEA (e.g. if disabled workers are simply lured away from small firms instead of being hired from non-employment). I investigate this issue in columns (4) and (8), in which also firms with firm size  $\in [5, 24]$  are included. In these specifications I interact the immediate as well as the short-run effect with an indicator function for firms not covered by the DPEA ( $= I(\text{firm size} < 25)$ ). The results are as follows. The immediate response for firms being subject to the non-compliance tax is 0.01837 in column (4) (without firm fixed effects) and 0.02902 in column (8) (with firm fixed effects), in each instance a value that is very close to the magnitude found in columns (1) and (5) respectively. The interaction term between the immediate response and an indicator function for firms not being subject to non-compliance taxation are of the same absolute magnitude as the effect for firms subject to non-compliance taxation, but with opposite sign. This suggests that there is no negative immediate impact for firms with less than 25 non-disabled workers that offsets the positive impact for firms with 25 or more employees. The exact same pattern is found for the short-run impact. This strongly supports the view that the tax increase has indeed a positive overall impact on the number of employed disabled workers.

To sum up and taking column (5) (linear time trend and firm fixed effects) as my preferred specification, I provide strong evidence that the tax increase led to a immediate as well as short-run response of firms covered by the DPEA. This impact is not offset by firms not covered by the DPEA. The immediate response of firms amounts to 0.0255, meaning that one in 40 firms employ one disabled more than they would without the tax increase (in terms of the average number of disabled workers, this is a 2.5% increase). After 18 month, and taking the short-run response into account, the effect amounts to 0.0669 ( $= 0.0255 + 18 \cdot 0.0023$ ). Thus, by the end of 2002, one in 15 firms employs one disabled more due to the tax increase (in terms of the average number of disabled workers, this is a 6.4% increase). This suggests that firms' elasticity of substitution between disabled and non-disabled workers equals 2.67 ( $= 6.4/2.4$ ).<sup>9</sup> This high substitutability is not surprising in the context of the DPEA. Recall that firms can recover the costs associated with the employment of a disabled worker (see section 4.2) and thus the productivity gap between disabled and non-disabled workers should not differ much. I conclude that the tax increase considerably increased firms' demand for disabled workers and thus policy makers aiming at boosting employment of disabled workers should favor a further rise in the non-compliance tax.

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<sup>9</sup>The average monthly wage firms pay to their workers is € 1953. Accordingly, in terms of the average monthly wage, the increase in the non-compliance tax by € 46 amounts to 2.4%.



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Table 4.1: The effect of the tax increase on the number of disabled workers per firm

	Number of disabled workers (adjusted for seasonality)																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																												
Mean	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.03727	1.0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Notes: (a) \*\*\*, \*\*, \* denotes significance at the 1%, 5%, and 10% level respectively. (b) Standard errors clustered by firm number. (c) All regressions include the full set of controls ( $X_i$ ) and firm size dummies ( $FSD_i$ ). (d) Sample: Private sector firms with 25-249 employees (columns (1)-(2) and (5)-(6)), with 31-249 employees (columns (3) and (7)), and with 5-249 employees (columns (4) and (8)). (e) Source: Own Calculations, based on ASSD and FWO



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### Recessions Are Bad for Workplace Safety

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Joint with Jan van Ours, Jan Boone, and Josef Zweimüller

#### 5.1 Introduction

Many workers face the risk of being involved in a workplace accident.<sup>1</sup> For instance, in the EU-15 in 2004 there were around 4 million occupational accidents leading to more than 3 days' absence from work, which is equivalent to an accident rate of 3.2%. The total number of accidents, including those which did not involve absence from work amounted to 6.4 million, equivalent to an accident rate of 5.3%.<sup>2</sup> The incidence of fatal accidents was 3.8 per 100,000 workers. Finally, annually around 140 million working days are lost due to non-fatal accidents. The accidents at work are estimated to cause annually costs of 55 billion Euros in EU-15, mostly due to lost working time.

Workplace accidents seem to be related to workplace safety, but cyclical fluctuations in workplace accidents are puzzling from an economic point of view. There are only few studies that address this question. Kossoris (1938) is a very early reference to the pro-cyclical pattern

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<sup>1</sup>According to the European Agency for Safety and Health at Work a workplace accident is defined as a “discrete occurrence in the course of work, which leads to physical or mental harm”. A fatal accident is defined as an accident, which leads to the death of a victim within 1 year (after the day) of the accident. The statistical information presented here is from European Commission (2008).

<sup>2</sup>There is a wide variation in the seriousness of the workplace accidents. Of all accidents in 2004, for 37% of accidents there was no absence from work or only up to three days, for 30% the absence was more than three days but less than two weeks and for 29% the absence was between two weeks and three months. Finally, the remaining 4% of accidents concerned an absence of three months or more, or permanent partial or total disability.

in accident rates. Fairris (1998) shows that in the U.S., manufacturing injury rates are pro-cyclical. Shea (1990) suggests that variables such as overtime, hiring and firing rates, the share of non-production workers, and the investment-to-capital ratio may affect the accident rate over the business cycle. If firms require more hours worked from employees in booms and less in recessions, then hours worked will be pro-cyclical and the accident rate (per worker) positively correlated with aggregate fluctuations in the economy.

It seems obvious that workplace accidents are pro-cyclical because effort and hours of work are negatively related to unemployment and high effort makes accidents more likely.<sup>3</sup> However, Boone and Van Ours (2006) provide an alternative explanation related to reporting behavior. Their idea is that in times of high unemployment workers are reluctant to report workplace accidents because they fear – correctly or incorrectly – that employers will hold this against them.<sup>4</sup> If they are fired in a recession, it will take them a long time to find a new job. Hence the worker prefers not to report an accident. One way to distinguish between the two explanations is to study cyclical fluctuations in fatal workplace accidents. If cycles in workplace safety drive the cycles in workplace accidents this should also be the case for fatal accidents, which are always reported. If reporting behavior of workers is relevant then fatal accidents should not be affected by the unemployment rate or changes in the unemployment rate. Using annual aggregate data from OECD countries Boone and Van Ours (2006) find that non-fatal workplace accidents are inversely related to the unemployment rate, while fatal accident rates do not seem to be related to labor market conditions, which suggests that workplace accidents are indeed influenced by reporting behavior.

This chapter studies cyclical fluctuations in workplace accidents using micro data. We have information on workplace accidents of male blue-collar workers from Austrian matched worker-firm data over the period 2000-2006. Our unique data allow us to investigate in great detail how economic incentives influence reporting of workplace accidents. The chapter is set up as follows. In Section 5.2 we present a theoretical model that explains the accident reporting behavior of individual workers. Workers are heterogeneous with respect to accident proneness and an accident reveals the innate probability of a worker to experience an accident. Workers report accidents because once reported firms invest in prevention. We show that workers become less eager to report accidents the higher the probability that the firm needs to fire a worker because then it may be more profitable for a firm to fire the worker rather than invest

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<sup>3</sup>Ruhm (2000) finds a strong relationship between macroeconomic conditions and mortality, which he attributes to hazardous working conditions, the physical exertion of employment, and job-related stress when job hours are extended during short-lasting economic expansions.

<sup>4</sup>OECD (1989) notes that among social and psychological factors which influence workplace accidents statistics that “workers may not report injuries because they fear loss of attendance bonuses, or other personal disadvantages, such as becoming prime candidates for redundancy”. Brooker *et al.* (1997) finding that back pain claim rates go down as unemployment goes up mention as possible explanation for this phenomenon that individuals choose to under-report claims during recessionary periods because they fear losing their jobs.

in prevention. We also show that more serious accidents are more often reported because the prevention of such accidents is more important for a worker. Section 5.3 describes the data on workplace accidents from our Austrian dataset. Section 5.4 presents some stylized facts, discusses the statistical model and presents estimation results. We find that workers who reported an accident in a particular period of time are more likely to be fired later on. Apparently, when deciding about whom to fire in case of a negative demand shock employers take the accident history of workers into account. And, we find support for the idea that recessions have a disciplinary effect concerning the reporting of workplace accidents: if workers think the probability of dismissals at the firm level is high, they are less likely to report a moderate accident. For severe accidents we find no such effect. Section 5.5 concludes.

## 5.2 Theory

We introduce a model in which workers who experience a work-related accident can decide whether to report the accident or not. If a worker reports the accident, the firm will make an investment to accommodate the workplace that reduces the probability of an accident to this worker in the next period. To see more precisely what we have in mind, consider the example of a nurse working in a hospital. One of her tasks is to lift people out of bed and help them into their wheelchair. Using the correct lifting techniques this can be done without causing back problems. However, the patient may lose his balance causing the nurse to overstretch her back. As a result, the nurse may (or may not) hurt her back. Having observed this accident, the firm may decide that the nurse is no longer allowed to lift patients out of bed on her own but instead has to get help from a colleague (or the beds of her patients are fitted with a device that facilitates lifting them).

More generally, think of a firm that obeys the general safety regulation rules and implements all accident prevention investments that are profitable to do for everyone. Nevertheless the firm may be willing to incur an additional cost that accommodates the individual's workplace once the firm has got additional information on the worker's individual accident risk. Of course, such workplace accommodation measures will be profitable for accident-prone workers but may not be profitable for workers with a small (individual) accident risk.

This is the main difference with the model in Boone and Van Ours (2006) where reporting an accident leads to a compensation (that varies with the severity of the accident) to the worker. In the model below, reporting the accident leads to investments to prevent future accidents. This makes the welfare analysis more interesting. Whereas there can only be under-reporting of accidents in Boone and Van Ours (2006), in the model below there can also be over-reporting of accidents.

### 5.2.1 Positive Analysis

Consider a two period model. In the first period, a firm has two workers. If an accident happens, a worker can report the accident by the end of the period. To simplify the exposition, assume that accidents are verifiable, once reported. Hence it does not make sense to report an accident that did not happen. At the end of the first period there is an (exogenous) probability  $\delta$  that demand for the firm's products falls which forces firm is forced to fire one of the two employees. Once the firm knows whether a worker stays or not, it can decide on worker-specific accident-prevention measures.

We assume that workers differ in type  $q \in [0, 1]$  where  $q$  denotes the innate probability of a worker to experience an accident. This probability  $q$  is distributed with density (distribution) function  $f(q)(F(q))$ . Before an accident happens, neither the worker, nor the firm knows  $q$ , only the distribution of  $q$ . If an accident happens, the worker experiences (expected) damage  $\alpha_w$  and the firm  $\alpha_f$ . We assume that the damage is independent of a worker's type. After the accident happened, the worker (but not the firm) learns  $q$ . The worker learns how likely he is to have a (similar) accident in the next period. While, after an accident, the worker's  $q$  is not observable the firm, the firm updates its beliefs about the worker's  $q$ .<sup>5,6</sup>

The firm can invest  $\gamma$  in worker specific prevention of the accident. After the investment, the probability that type  $q$  is involved in an accident is denoted by  $q_\gamma \leq q$  where we assume that

$$\frac{d(q - q_\gamma)}{dq} > 0 \quad (5.1)$$

The prevention technology leads to a bigger fall in the accident probability for more accident prone workers. Basically, there is more to gain for high  $q$  workers.<sup>7</sup> We assume that it is not profitable to invest in the accident prevention technology for every worker:

$$E(q - q_\gamma)\alpha_f \leq \gamma \quad (5.2)$$

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<sup>5</sup>Assuming that the worker learns  $q$  (perfectly) simplifies notation. The important feature is that the worker's posterior distribution of  $q$  is more informative than the firm's posterior.

<sup>6</sup>In fact, after the accident the firm would like to learn  $q$ . One way to screen workers on  $q$  after the accident is to offer them a choice between receiving an amount of money ("bribe") or the firm investing in safety measures. To the best of our knowledge, offering such a choice to workers is illegal.

<sup>7</sup>To illustrate things, consider again the nurse in the hospital. The patient may lose his balance causing the nurse to overstretch her back with probability  $\lambda$ . Given that this situation arises, the nurse's has individual probability  $\tilde{q}$  that she hurts her back, hence the ex-ante probability of an accident is  $q = \lambda\tilde{q}$ . After the accident the nurse knows whether her back is likely to be strained again in a similar future situation ( $\tilde{q}$  close to 1). Alternatively, she may have hurt herself at home the day before which weakened her back. Therefore, although she hurt her back in this incident she is unlikely to get hurt in a similar situation in the future. When the hospital decides that the nurse is no longer allowed to lift patients out of bed on her own but has to get help from a colleague, the probability of ending up in a hazardous situation is reduced from  $\lambda$  to  $\lambda_\gamma < \lambda$ . Indeed we find that  $d((\lambda - \lambda_\gamma)\tilde{q})/d\tilde{q} > 0$  in this example.



where  $E(q) = \int qdF(q)$  denotes the (prior) expected probability of an accident.

Now consider the case where a worker has reported an accident. The posterior distribution is then given by

$$f(q|A) = \frac{qf(q)}{\int_0^1 tdF(t)} \quad (5.3)$$

We assume that it is profitable to invest in prevention, once a worker has reported an accident:

$$E(q - q_\gamma|A)\alpha_f > \gamma \quad (5.4)$$

where  $E(q|A) = \int qdF(q|A)$ . In words, once a worker has reported an accident, the firm knows that the worker is accident prone and investment in prevention becomes profitable. This effect gives an incentive for a worker to report an accident (see below). Note that the firm makes its investment decision without knowing  $q$  for the worker since  $q$  is not revealed to the firm. We also assume that it is not profitable to replace a worker who has reported an accident in the first period. Let  $C$  denote the firing and rehiring cost. It is assumed that, a worker who produces surplus  $y$  and receives wage  $w$ , we always have

$$y - w - \gamma - E(q_\gamma|A)\alpha_f > -C + y - w - E(q)\alpha_f. \quad (5.5)$$

That is, investing  $\gamma$  and reducing the probability of an accident is more profitable than investing search cost  $C$  and employing a new worker who has an accident with expected probability  $E(q)$ .

If a worker loses the job, he or she receives  $b < w$  as expected value of being unemployed. This consists of a probability of finding a new job in the next period<sup>8</sup> and unemployment benefit if no other job is found. We assume that a worker prefers to stay in his job at the start of the first period:

$$w - E(q)\alpha_w > b \quad (5.6)$$

Similarly, every type  $q$  prefers to stay in the second period, if the firm invests in prevention:

$$w - q_\gamma\alpha_w > b \quad (5.7)$$

for each type  $q_\gamma$ . In other words, no worker voluntarily quits after reporting an accident.

Now consider the worker's incentive to report an accident in the first period. On the one hand, reporting the accident has a potential benefit to the worker. If the accident is reported, it is optimal for the firm (equation (5.4)) to invest in workplace accommodation which reduces the accident probability for the worker from  $q$  to  $q_\gamma$ . On the other hand, reporting the accident

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<sup>8</sup>To ease notation, we assume that the probability of having an accident in the next job –in case the worker is fired– does not depend on a worker's current  $q$ . That is,  $q$  is specific to the worker-firm match.

has also a potential cost. If the firm is hit by a negative demand shock (which happens with probability  $\delta$ ), the firm is better off firing the worker. (If either none or two workers reported an accident, the firm is indifferent in firing either employee. In that case, an employee is chosen with probability  $\frac{1}{2}$ .<sup>9</sup>)

Given that a worker is the only one who experienced an accident in the first period, his expected pay off from reporting is given by

$$\delta b + (1 - \delta)(w - q_\gamma \alpha_w) \quad (5.8)$$

If the negative shock hits the firm, it will fire the worker who has reported the accident, save on the accommodation expenditure  $\gamma$  and get a lower probability of future accidents for the remaining worker (as  $E(q|A) > E(q)$ ). If the worker is fired, he receives  $b < w$ . If the worker does not report the accident, his pay off equals

$$\frac{1}{2}\delta b + (1 - \frac{1}{2}\delta)(w - q\alpha_w) \quad (5.9)$$

Hence a worker reports if and only if the expression in equation (5.8) exceeds (5.9); which can be rewritten as

$$\frac{1}{2}\delta q + (1 - \delta)(q - q_\gamma) > \frac{1}{2}\frac{\delta(w - b)}{\alpha_w} \quad (5.10)$$

Because of assumption (5.1), the left hand side is increasing in  $q$ . Hence there is a critical value  $q^*$  such that the inequality is satisfied for all  $q > q^*$ . Thus the (ex-ante) probability that the worker reports an accident is  $1 - F(q^*)$ .

We find the following result.

**Proposition 1** *As  $\delta$  increases, the probability that an accident is reported falls. As  $\alpha_w$  increases, it becomes more likely that an accident is reported. As  $\alpha_w$  increases, the effect of  $\delta$  on  $q^*$  is reduced.*

**Proof of proposition 1** Let  $q^*$  denote the value for  $q$  where (5.10) holds with equality. Then it is routine to verify that

$$\frac{dq^*}{d\delta} = \frac{\frac{w-b}{2\alpha_w} - \frac{1}{2}q_\gamma^* + \frac{1}{2}(q^* - q_\gamma^*)}{\frac{1}{2}\delta + (1 - \delta)\frac{d(q^* - q_\gamma^*)}{dq^*}} > 0 \quad (5.11)$$

where the inequality follows from equations (5.1), (5.7) and  $q - q_\gamma > 0$ . If  $\alpha_w$  increases, the right hand side of (5.10) falls and  $q^*$  falls as well. Finally, an increase in  $\alpha_w$  reduces  $dq^*/d\delta$  in equation (5.11). Hence,  $q^*$  is less dependent on  $\delta$  as  $\alpha_w$  increases. *Q.E.D.*

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<sup>9</sup>Note that in the second (final) period it is immaterial whether a worker reports an accident or not. No further accidents can be prevented and there is no firing decision by the firm.

The intuition is as follows. The higher the probability that the firm needs to fire a worker, the less eager workers become in reporting accidents. More serious accidents (higher  $\alpha_w$ ) are more often reported, since the prevention of such accidents is more important for a worker. Finally, the reporting of more serious accidents is less dependent on the expectations about the firing rate  $\delta$ .

Notice that our simple model yields empirically testable prediction which will be analyzed in the empirical analysis below. In particular, we will not only investigate whether accident reporting increases in booms and decreases in regressions but we will also test whether the cyclicity in reporting behavior is different between moderate and severe workplace accidents.

### 5.2.2 Welfare Analysis

We conclude the section by summarizing the normative implications of the model. There are two main imperfections that cause the equilibrium above to deviate from the socially optimal outcome. First, both the worker and the firm only consider their own payoffs not the sum of payoffs. To illustrate, equation (5.2) implies that it is not profitable for the firm to invest for every worker. However, if we have

$$E(q - q_\gamma)(\alpha_f + \alpha_w) > \gamma \quad (5.12)$$

it is socially desirable that such an investment is made (ex ante) for every worker. Second, after an accident the worker learns his own  $q$  but cannot credibly reveal this to the employer. Hence the firm bases its investment decision on  $f(q|A)$  while it is socially optimal to base this decision on  $q$ . Related to this, the decision to fire a worker (with probability  $\delta$ ) after an accident should also be based on  $q$  and not on the accident itself. If the worker has accident probability  $q < E(q)$  the social planner prefers the workplace accommodating investment to firing the worker. However, when  $q > E(q)$  even the social planner prefers to fire a worker rather than investing in prevention of a further accident.

In the model above, reporting an accident leads the firm to invest in worker-specific safety measures (if the worker is not fired). Hence for all  $q > q^*$  (where  $q^*$  solves equation (5.10) with equality) the firm invests. Given that a worker is not fired after being involved in an accident, for which values of  $q$  is it socially optimal to invest?

**Lemma 2** *It is socially optimal to invest in safety measures for a worker with  $q > q^s$  where  $q^s$  is defined by*

$$(q^s - q_\gamma^s) = \frac{\gamma}{\alpha_w + \alpha_f} \quad (5.13)$$

Since we assume that  $q - q_\gamma$  is increasing in  $q$ , there is a unique value for  $q^s$  that satisfies equation (5.13). For  $q < q^s$  the gain in safety  $(q^s - q_\gamma^s)(\alpha_w + \alpha_f)$  due to the investment is too

small to cover the cost  $\gamma$ .

**Proposition 3** *Assume that the solution  $q^s$  to equation (5.13) satisfies*

$$q^s < \frac{w - b}{\alpha_w}$$

*then there exists  $\delta^* \in \langle 0, 1 \rangle$  such that for  $\delta < \delta^*$  the firm over-invests in safety measures compared to the social optimum while for  $\delta > \delta^*$  the firm under-invests.*

**Proof of proposition 3** *First*, consider  $\delta = 0$ . Then equation (5.10) implies that each worker  $q \in [0, 1]$  reports his accident, inducing the firm to invest in safety. However, such investment is only optimal for  $q > q^s > 0$ . Hence for low  $\delta$  we have over-investment in safety measures. *Second*, consider  $\delta = 1$ . Comparing equations (5.8) and (5.9), it follows that workers  $q$  report who satisfy

$$w - q\alpha_w < b$$

That is, workers who prefer to be unemployed rather than to continue working without additional safety measures report their accident (and get fired with probability 1). If it is the case that

$$w - q^s\alpha_w > b$$

type  $q^s$  does not report in equilibrium while this would be socially optimal. Hence under the assumption made in the proposition, there is under-reporting and hence under-investment for  $\delta$  close to 1. *Finally*, note that  $q^s$  does not depend on  $\delta$  while equation (5.11) implies that  $q^s$  is increasing in  $\delta$ . Hence there exists  $\delta^* \in \langle 0, 1 \rangle$  such that there is over(under)-investment for  $\delta > (<) \delta^*$ . *Q.E.D.*

The intuition for this result is as follows. When the probability of being fired is small, workers report each accident thereby inducing the firm to invest even though they know their probability of having another accident  $q$  is actually quite low. However, for  $\delta$  close to zero, there are no costs for the worker of doing this (probability of being fired is close to zero) and the firm bases its decision on the posterior distribution (5.3) not on the realization of  $q$ .

When the probability of being fired is high, workers do not report even though they learned that the probability of an accident  $q$  and therefore the gain from prevention  $q - q_\gamma$  is high. In this case, the firm does not invest even though such investment would be socially optimal. The interpretation of this result is as follows. Recessions are bad for workplace safety in the sense that they lead to under-reporting of accidents and therefore to underinvestment in prevention. In booms there is over-investment in workplace safety. Thus booms are “too good” for workplace safety. Accident-prevention investments are made that are wasteful from a social point of view but beneficial for workers.

## 5.3 Data and Key Variables

### 5.3.1 Data

Our data are from the Austrian Social Insurance for Occupational Risks (AUVA) which covers all employees except federal railway employees and civil servants (2.8 million). The AUVA defines an occupational accident as “an unexpected external event causing injury, in locational, temporal and causal relationship to the insured occupation”.<sup>10</sup> By law, occupational accidents, due to which an insured person is more than 3 days incapable of working – including fatal accidents – are required to be reported within 5 days. In 2007, roughly 110,000 occupational accidents were reported. In case of an occupational accident the employer is legally obligated to continued remuneration of the victim for 8-10 weeks, depending on the job tenure of the worker. The cost of the curative treatment associated with the occupational accident is covered by the AUVA (the 2007 budget amounted to € 950 million).

The mandatory nature of the accident insurance implies that the AUVA keeps track of all reported workplace accidents. The data available to us covers workplace accidents that were reported between 2000 and 2006.<sup>11</sup> In addition to the exact accident date, the AUVA data contains detailed information on the severity of accidents along four dimensions: (i) the number of inpatient care days, (ii) the number of days absent from work (excluding inpatient care days), (iii) whether the accident caused a reduction in the ability to work (i.e. whether the victim receives a (partial) disability pension as a result of the accident), and (iv) whether the accident is fatal. Moreover, it includes some information on the firm (such as industry affiliation) and the worker (such as whether a worker holds a blue- or white-collar occupation).

The unique feature of our dataset consists of a personal identifier which allows us to match the workplace accidents to individual data from the Austrian Social Security Database (ASSD, see *Zweimüller et al.* (2009) for details). The ASSD contains detailed information on individuals’ employment status and earnings on a daily basis. It also contains some information on the employer, like regional location, industry affiliation, and a firm identifier. This enables us not only to derive the employment history of each individual, but also to characterize every single firm at each reference date in terms of its firm size, employment flows, and mean characteristics of their employees.<sup>12</sup>

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<sup>10</sup>Activities in connection with the insured occupation, e.g. commuting to and from the workplace are covered by this insurance as well.

<sup>11</sup>More precisely, the data contains all workplace accidents of which the corresponding claims were recognized in 2000-2007. There is a time lag between reporting and recognition of accidents. For example, 93.0 % of claims in 2000, were recognized in the same year and another 6.6 % in the subsequent year. So, almost all claims are dealt within the same or the subsequent year. Because of this we ignore the workplace accidents that occurred in 2007, because for this year the records may be incomplete.

<sup>12</sup>Note, however, that 3.4 % workplace accidents cannot be matched unambiguously. This occurs when an individual is employed at more than one employer at once. Workplace accidents for those individuals cannot

We restrict our sample to male blue-collar workers aged 25–49 who are employed in manufacturing with at least 100 employees. In this sample, 97.8 % of occupational accidents observed in the AUVA data could be matched to a corresponding employment record in the ASSD data. This sample is the most relevant with regard to occupational accidents.<sup>13</sup> In the empirical analysis we work with longitudinal data (monthly reference dates) containing individuals that are employed at the reference date. For this sample, a total of 64,080 workplace accidents are reported in the AUVA data during the period 2000–2006.

### 5.3.2 Definition of Key Variables

The two crucial variables for the empirical analysis are (i) a measure for the degree of severity of an occupational accident and (ii) a measure that captures the probability that demand for the firm’s products falls  $\rho$ . These variables are defined as follows. We distinguish two types of accidents, *moderate* and *severe* ones. This distinction should capture whether or not the reporting of an occupational accident is at discretion of the workers. In regard of this, we define an accident to be moderate if it simply results in a positive number of days absent from work (excluding inpatient care days). We define an occupational accident as *severe* otherwise, i.e. if it results in a positive number of inpatient care days, in a (partial) disability pension, or if it is fatal. This classification is a robust one for two reasons. First, it is implausible that severe accidents are over-reported, since a worker would have to fake an injury that brings him into hospital or one that makes him eligible to a partial disability pension. Second, *severe* accidents are very unlikely to be under-reported, since the experienced damage to the worker  $\alpha_w$  is likely to be very high (high enough to end up in hospital or to suffer from a permanent reduction in work capacity). In contrast, our measure of moderate accidents is potentially subject to a reporting decision.

We define the measure for the probability that a firm will face a fall in demand for their products  $\rho$  as the fraction of the workforce a firm lays off from  $t - 1$  to  $t$ . We therefore assume that the workers’ best predictor at  $t$  for the probability of an adverse demand shock in  $t + 1$  is the firm’s human resource planning (i.e. firing decisions) in  $t$ . We define “fired” as

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be directly linked to an employer, since the AUVA data does not contain a firm identifier. We applied the following procedure in case of an unambiguous match: we matched the workplace accident to that employment spell, which was – based on information that is available in both data sets – the most likely. More precisely, we chose the most likely match by first ensuring consistency of the industry classification, second ensuring consistency of the type of employment (blue – vs. white-collar), and finally, if the match was still ambiguous, we simply chose the longest employment spell in question.

<sup>13</sup>The following figures illustrate this (all figures refer to the year 2006): First, the manufacturing industry is the largest in terms of the number of employees accounting for roughly 18 % of the overall number of employees. Second, female workers only account for 28 % in the manufacturing industry. Third, this industry accounts by far for the largest share of occupational accidents (25 % or 30,000). Fourth, roughly two-third of occupational accidents in the manufacturing industry concern male blue-collar workers.

a worker who is employed in  $t - 1$  and either unemployed or out-of-labor force in  $t$ .<sup>14</sup> Note that the firing rate is based on the entire workforce of each firm (i.e. including workers of all ages, white-collar workers, women, and industries other than manufacturing) and not on our restricted sample. Moreover, all regressions will additionally include a variable capturing the general business cycle measured by the monthly unemployment rate for male workers in manufacturing at the state level as well as calendar month and year dummies. Thus, the cyclical effects entirely arise from idiosyncratic variation in firms' firing rates.

### 5.3.3 Descriptive Statistics

Our final sample contains 64,080 workplace accidents, of which 93.9% were moderate and 6.1% were severe. Table 5.1 shows that our final data contains 9,263,282 individual-month observations (based on 205,170 workers who work in 1,256 different firms). Each worker has on average a risk of 0.7 percent to get involved in an occupational accident at each reference date, which is rather a low number. Considering the whole 2000–2006 period changes this picture. Almost one-third of all workers report at least one occupational accident over this period. Firms have a mean firing rate of 1.3 percent. The average unemployment rate in the manufacturing industry for male workers is 5.0 percent. The average worker can be characterized as follows: he is aged 37.4 years, employed in the same firm since 9.2 years, has work experience of 18.1 years, was on sick leave for 0.3 percent in the last 2 years, and earns € 81.2 a day. Table 5.2 provides companion information at the level of the firm (i.e. the average values of their workforce of male, blue-collar workers aged 25–49 years).

## 5.4 Empirical Analysis

### 5.4.1 Descriptive Evidence

To give some idea about the relationships between accident rates and the business cycle, Panel A of Figure 1 plots the probability of a firm being confronted with at least one workplace accident against the distribution of the firing rate at the firm level (in brackets of 5 %-quantiles) and Panel B of Figure 1 plots the unemployment rate at the state level (in brackets of 10 %-quantiles). The top graph of Figure 1 shows that if the firing rate is within the bottom 25 % quantile (conditional on being positive), a firm faces a risk of 39–61 percent that at least one moderate accident is reported (the solid line and left axis).<sup>15</sup> This number

<sup>14</sup>A worker who is employed and then either moves to another firm or gets retired (old-age or disability-related) is not considered to being fired.

<sup>15</sup>Only 29 percent of firms are confronted with an accident if they do not fire any worker (20% of the firm-month observations exhibit a firing rate of zero). At first, this unexpectedly low accident rate at the very lower tail of the firing rate distribution is puzzling. However, this low probability can be explained by a purely



Table 5.1: Summary statistics at individual level

Variable	Mean (Standard deviation)
Accident in $t$ (yes / no)	0.0069 (0.0829)
Accident in 2000–2006 (yes / no)	0.3293 (0.4700)
Firing rate (from $t - 1$ to $t$ )	0.0128 (0.0192)
Unemployment rate	0.0504 (0.0281)
Age	37.3941 (6.8500)
Tenure (in 10 years)	0.9221 (0.7194)
Experience (in 10 years)	1.8069 (0.7423)
Sickness rate (last 2 years)	0.0031 (0.0207)
Daily wage (in €)	81.2243 (18.3017)
Number of worker-month observations	9,263,282
Number of workers	205,170
Number of firms	1,256

Notes: Standard deviation in parentheses. Sample Selection: male, blue-collar workers, aged 25–49 years and employed in firms (manufacturing sector) with on average at least 100 employees over the sample period from 2000–2006. Source: Own Calculations, based on AUVA, ASSD, and BMWA

Table 5.2: Summary statistics at firm level

Variable	Mean (Standard deviation)
Moderate accident in $t$ (yes / no)	0.3698 (0.4828)
Severe accident in $t$ (yes / no)	0.0411 (0.1985)
Number of male, blue-collar workers, aged 25-49	107.0528 (194.9790)
Firing rate (from $t - 1$ to $t$ )	0.0153 (0.0275)
Unemployment rate	0.0529 (0.0306)
Age	37.3478 (2.1379)
Tenure (in 10 years)	0.8414 (0.4264)
Experience (in 10 years)	1.7413 (0.3316)
Sickness rate (last 2 years)	0.0028 (0.0048)
Daily wage (in €)	74.6025 (13.6947)
Number of firm-month observations	86,530
Number of firms	1,256

Notes: Standard deviation in parentheses. Sample Selection: Firms (manufacturing sector) with on average at least 100 employees over the sample period from 2000–2006. All variables refer to the male, blue-collar workforce aged 25-49. Source: Own Calculations, based on AUVA, ASSD, and BMWA

amounts to 5–10 percent for severe accidents (the dashed line and right axis). The pattern for moderate accidents is striking. The firm’s risk that at least one moderate accident is reported is decreasing with the expected firing rate. At the highest 5 % quantile, the risk has decreased to 32 percent. In contrast, this risk remains fairly stable over the entire distribution of the firing rate for severe accidents (fluctuating around roughly 4 percent). For the unemployment rate (Panel B of Figure 1) the same pattern emerges. This *prima-facie* evidence strongly supports the hypotheses underlying our theoretical model.

### 5.4.2 The Empirical Model

This section describes the empirical methodology. We focus on two main issues. First, our empirical analysis will address the question whether workers who report an accident are in fact exposed to a higher risk of subsequent job loss. Second, we empirically investigate, whether reported workplace accidents vary over the business cycle and how the cyclicity in reporting behavior differs between severe and more moderate workplace accidents. This second analysis forms the basis of our empirical investigation.

The following regression equation allows us to test whether workers who have recently reported an accident are those that are fired with a higher probability

$$y_{it+1} = \kappa \cdot pra_{it} + X_{it}^{f'} \cdot \beta + \psi_i^f + \phi_{j(i)t}^f + \pi_t^f + \nu_{it} \quad (5.14)$$

where  $y_{i+1t}$  indicates whether or not a worker  $i$  is fired in period  $t+1$  as a function of current and past accidents that worker  $i$  has reported at his current firm  $j$  ( $pra_{it}$ ), control variables  $X_{it}^f$ , worker fixed-effects ( $\psi_i^f$ ), firm fixed-effects ( $\phi_{j(i)t}^f$ ), and calendar month and year dummies ( $\pi_t^f$ ). The control variables  $X_{it}^f$  include a constant, the state unemployment rate for male, blue-collar workers, the sickness rate of the last 2 years of worker  $i$  in firm  $j$  (and its square), the daily wage (in logs), age (and its square), tenure (and its square), and experience (and its square).  $\nu_{it}$  is an error term satisfying the usual assumption which captures unobservable (to the researcher) factors influencing the reporting of occupational accidents. The prediction from our theoretical model is correct if the sign of  $\kappa$  is positive.

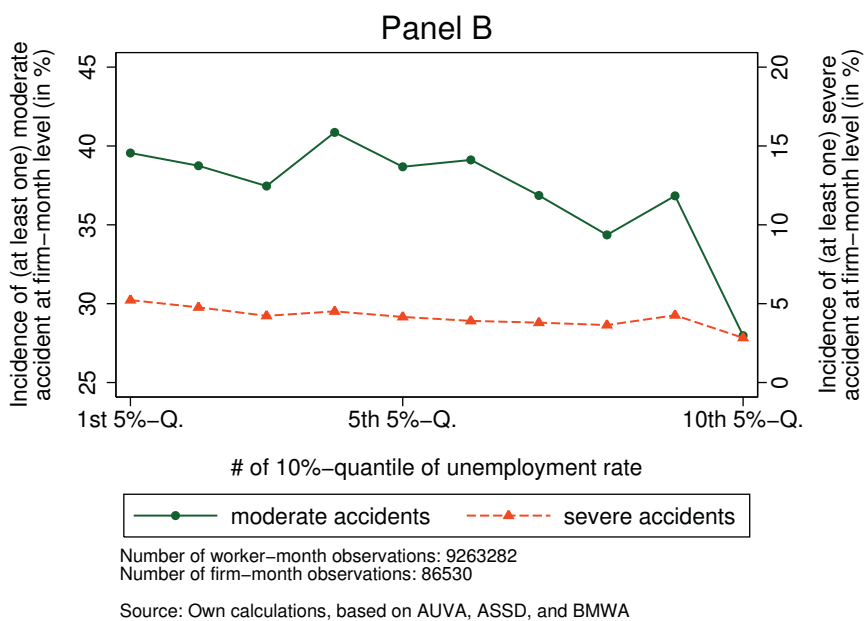
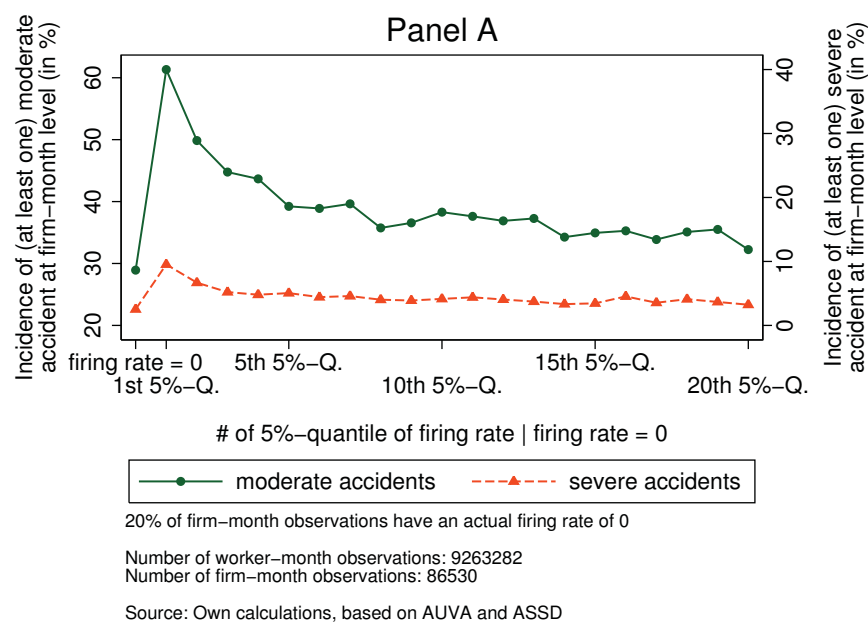
The following regression equation studies the cyclical behavior of accident reporting

$$a_{it} = \delta \cdot f_{j(i),t} + X_{it}^{a'} \cdot \beta + \psi_i^a + \phi_{j(i)t}^a + \pi_t^a + \epsilon_{it}, \quad (5.15)$$

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statistical reasoning (note that the figure is unconditional on any observables). Firm-month observations with a firing rate of zero are only half of the size as firm-month observations with a positive firing rate (178 versus 315 employees). *Ceteris paribus*, firm-month observations with a firing rate of zero have a 50% lower probability of being confronted with an accident. Adjusting for that would increase the accident rate to roughly 58% for such firm-month observations, which then results in a figure perfectly consistent with our theory.

Figure 5.1: Plot of incidence of at least one accident at firm-month level against the firing rate (Panel A) and the unemployment rate (Panel B)



where  $a_{it}$  indicates whether individual  $i$  reports an accident in  $t$ ,  $f_{jt}$  is the firing rate of firm  $j$  at time  $t$ ,  $\psi_i^a$  are worker fixed-effects,  $\phi_{j(i)t}^a$  are firm fixed-effects and  $\pi_t^a$  represent calendar month and year dummies. The control variables  $X_{it}^a$  include a constant, the sickness rate of the last 2 years of worker  $i$  in firm  $j$  (and its square), the daily wage (in logs), age (and its square), tenure (and its square), and experience (and its square), and  $\epsilon_{it}$  is an error term satisfying the usual assumption which captures unobservable (to the researcher) factors influencing the reporting of occupational accidents. We estimate this regression equation separately for moderate and severe occupational accidents using all worker-month observations with a moderate occupational accident together with worker-months observations without any accident, and all worker-month observations with a severe occupational accident together with worker-months observations without any accident.

### 5.4.3 Empirical results

**Does reporting an accident increase the subsequent firing rate?** We start our analysis by investigating whether the workers who reported an accident are exposed to a higher risk of job loss as compared to workers who did not report an accident. The existence of a significant impact of a previous accident on the risk of subsequent job loss is of obvious importance in the present context. Our theoretical reasoning builds upon the idea that an incentive not to report a workplace accident arises exactly because accident reporting may increase the subsequent probability of getting fired. If the accident reporting behavior would be unrelated to the risk of job loss, the mechanism emphasized in this chapter would not be relevant.

Table 5.3 shows the results of the impact of previous accident reporting (within the last twelve months) on the current probability of job loss. Columns (1)–(4) use the overall accident reporting during the last 12 months as the independent variable. Column (5) additionally allows for differential effects by the degree of severeness. It turns out that reporting any accident in the previous year, leads to a substantial increase in the probability of being laid off in the current month (column (1)). This effect is robust to the inclusion of worker fixed effects (column (2)), firm fixed effects (column (3)), and the inclusion of both worker as well as firm fixed effects (column (4)). Taking column (4) as our preferred specification (controlling for firm and worker characteristics that persist over time), the point estimate suggests that the firing probability is 0.15 percentage points higher for workers who have reported any accident within the last twelve months. This effect compares to an average firing rate of 0.75 percent in our sample. Hence we conclude that the impact of accident reporting on the risk of subsequent job loss is substantial, and equal to roughly 20 percent of the average firing rate in the sample. When we allow for differential impacts of moderate and severe accidents in Column (5) we see that the effect is mainly driven by moderate accidents whereas the impact

of previous severe accidents is insignificant. The fact that severe accidents do not have an impact on subsequent job loss may seem surprising. However, workers who have a severe accident may temporarily be absent from their job but still count as employees of the firm due to job protection legislation. In sum, our results strongly support the assumption made in section 5.2, that accident reporting behavior has a significant impact on the probability of being laid off later on.

In both specification we also see that the unemployment rate has a significant effect on the individual probability to be fired. This effect is not surprising and represents a business cycle effect. If the economy is in a recession workers are more likely to loose their job. The impact is quantitatively important. A one percentage point increase in the contemporaneous unemployment rate leads to a 21 percent higher probability to be fired ( $= [0.1589/100]/0.0075$ ). All five specifications also show that a higher sickness rate within the last two years leads to a higher firing probability. This suggests that a similar mechanism as for accident rates may be at work for sickness rates.<sup>16</sup>

**Does accident reporting behavior vary over the business cycle?** Our estimate of main interest concerns the cyclicity of accident reporting behavior. We report results of the effect of the firing rate on the probability to report an accident in table 5.4 where separate parameter estimates for moderate (Panel A) and severe (Panel B) accidents are presented. We report results on four different specifications that differ by the inclusion of worker and/or firm fixed effects. If neither worker nor firm fixed-effects are included (column (1)), then the effect of the firing rate on the probability of reporting a moderate accident is not significantly different from zero. When we include worker fixed effects, in contrast, we do find a significant negative effect. An increase of the firing rate by one standard deviation ( $= 0.0192$ , see Table 5.1) decreases the probability of reporting a moderate accident by 2.1 percent ( $= [0.0192 \cdot (-0.0072)]/0.0065$ ). Apparently, adding worker fixed-effects (e.g. controlling for workers' ability to prevent a moderate workplace accident) is important. Put differently (and assuming that worker fixed effect are the only left-out confounders in column (1)), workers' time-invariant traits that reduce the probability of reporting a moderate workplace accident are negatively correlated with the firing rate. Column (3) replaces the worker fixed-effects by firm fixed-effects, which control *inter alia* for job safety measures provided by the employer. It turns out that the effect is now also significantly different from zero. This implies (assuming that the only left-out confounders are firm fixed effects in column (1)) that any workplace characteristics provided by the employer that reduce the probability of reported moderate accidents by workers are negatively correlated with the firing rate. The results in

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<sup>16</sup>See for example Barmby *et al.* (1994) who indicate that the effect of absence behavior on the probability of being fired may act as a worker discipline device.

Table 5.3: Regression results for testing the crucial model assumption

	Dummy variable (= 1 if worker is laid off)				
Mean	0.0075				
Standard deviation	0.0861				
moderate accident in $t \in [t - 12, t - 1]$	0.0016*** (0.0001)				
severe accident in $t \in [t - 12, t - 1]$	0.0002 (0.0006)				
any accident in $t \in [t - 12, t - 1]$	0.0011*** (0.0001)	0.0015*** (0.0001)	0.0008*** (0.0001)	0.0015*** (0.0001)	
unemployment rate	0.0189*** (0.0016)	0.1386*** (0.0073)	0.0833*** (0.0068)	0.1589*** (0.0070)	0.1589*** (0.0070)
sickness rate (last 2 years)	0.0778*** (0.0052)	0.0436*** (0.0061)	0.0637*** (0.0050)	0.0413*** (0.0061)	0.0419*** (0.0061)
sickness rate (last 2 years) <sup>2</sup>	-0.0655*** (0.0188)	-0.0028 (0.0232)	-0.0455** (0.0185)	0.0016 (0.0232)	0.0007 (0.0232)
Worker fixed-effects	No	Yes	No	Yes	Yes
Firm fixed-effects	No	No	Yes	Yes	Yes
Number of worker-month obs.	7,883,354				

Notes: (a) \*\*\*, \*\*, \* denotes significance at the 1%, 5%, and 10% level respectively. (b) All regressions (linear probability model) include the following control variables (not shown in the table): calendar month dummies, year dummies, age (and its square), tenure (and its square), experience (and its square), logarithm of daily wage (in €). (c) Sample selection: male, blue-collar workers, aged 25–49 years and employed in firms (manufacturing sector) with on average at least 100 employees over the sample period from 2000–2006. (d) Robust standard errors in parentheses (adjusted for clustering on workers). (e) Sample is restricted to the years 2001–2006 due to the 1 year lag in workers' workplace accidents. (f) Source: Own Calculations, based on AUVA, ASSD, and BMWA.



column (2) and column (3) as compared to column (1) are in line with empirical findings in the compensating wage differential literature that more able workers sort into jobs with positive job characteristics (see e.g. Hwang *et al.* (1992)). Column (4) includes worker as well as firm fixed effects, which is our preferred specification. Column (4) suggests that increasing the firing rate by one standard deviation leads to a 2.1 percent decrease in the probability to report a moderate accident. Notice further that all regressions in Table 5.4 control for the general business cycle (by including a dummy for each calendar year as well as the monthly unemployment rate for male workers in manufacturing at the state level) as well as for seasonal effects (by including a dummy for each calendar month). Hence the estimated cyclical effects arise from idiosyncratic variations in the firing rates, holding the business cycle and seasonal effects constant. The relationship between the firing rate and the reporting behavior of *severe* workplace accidents is investigated in Panel B of table 5.4. We perform exactly the same regressions as we do for moderate workplace accidents. The results show that the firing rate does not have any statistically significant impact on workers' reporting behavior with respect to severe workplace accidents. This finding strongly supports our alternative explanation of the cyclical fluctuation in workplace accident rates.

The larger a firm is the higher the risk that at least one occupational accident is reported, simply because they employ more workers. In addition, larger firms generally tend to have lower firing rates, which may suggest a mechanical interpretation for the negative relationship between the incidence of an accident and the expected firing rate. Therefore, we also performed an analysis at the level of the firm. Table 5.5 shows the parameter estimates. We also perform a sensitivity analysis with respect to the inclusion of firm fixed effects as already done in table 5.4 by presenting results without including firm fixed effects (column (1)) and with including firm fixed effects (column (2)). It turns out that the results based at the level of the firm are very much in line with the estimates based on individual data. In column (1) we again see that the firing rate is unrelated to the probability of reporting an moderate accident if firm fixed effects are not controlled for. Including firm fixed effects changes the picture. Column (2) shows that an increase in the firing rate by one standard deviation ( $= 0.0275$ , see table 5.2) reduces the probability that a firm faces at least one moderate workplace accident by 1.5 percent ( $= [0.0275 \cdot (-0.2066)]/0.3698$ ). The probability that a firm faces at least one severe accident, in contrast, is unrelated to the firing rate. The analysis at the level of the firm thus strongly supports our findings derived at the level of the worker.

To sum up, the results of Tables 5.3–5.5 are consistent with our theoretical predictions. Workers who report a workplace accident in the previous year are more likely to be laid off. This suggests that when deciding about whom to fire employers take the accident history of workers into account. We have also shown that while the probability of reporting a *moderate* accident is governed by firms' firing rate, this is not true for *severe* accidents. Recall that

Table 5.4: Regression results of the impact of the firing rate on accident reporting (at individual level)

<b>Panel A: Moderate Workplace Accidents</b>				
	Dummy variable (= 1 if worker reports an accident)			
Mean	0.0065			
Standard deviation	0.0804			
Firing rate (from $t - 1$ to $t$ )	0.0006 (0.0016)	-0.0072*** (0.0017)	-0.0084*** (0.0017)	-0.0074*** (0.0017)
Worker fixed-effects	No	Yes	No	Yes
Firm fixed-effects	No	No	Yes	Yes
Number of worker-month observations	9,259,374			

<b>Panel B: Severe Workplace Accidents</b>				
	Dummy variable (= 1 if worker reports an accident)			
Mean	0.0004			
Standard deviation	0.0206			
Firing rate (from $t - 1$ to $t$ )	0.0008 (0.0005)	-0.0005 (0.0006)	-0.0003 (0.0006)	-0.0005 (0.0006)
Worker fixed-effects	No	Yes	No	Yes
Firm fixed-effects	No	No	Yes	Yes
Number of worker-month observations	9,203,110			

Notes: (a) \*\*\*, \*\*, \* denotes significance at the 1%, 5%, and 10% level respectively. (b) All regressions (linear probability model) include the following control variables: calendar month dummies, year dummies, age (and its square), tenure (and its square), experience (and its square), logarithm of daily wage (in €), sick leave rate in the previous two years (and its square). (c) Sample selection: male blue-collar workers, aged 25–49 years and employed at firms (manufacturing sector) with on average at least 100 employees over the sample period from 2000–2006. (d) Robust standard errors in parentheses (adjusted for clustering on workers). (e) Source: Own Calculations, based on AUVA and ASSD.

Table 5.5: Regression results of the impact of the firing rate on accident reporting (at firm level)

<b>Panel A: Moderate Accidents</b>		
	Dummy variable (= 1 if firm faces an accident)	
Mean	0.3698	
Standard deviation	0.4828	
Firing rate (from $t - 1$ to $t$ )	-0.0212 (0.0793)	-0.2066*** (0.0743)
Firm fixed-effects	No	Yes
Number of firm-month observations	86,530	

<b>Panel B: Severe Accidents</b>		
	Dummy variable (= 1 if firm faces an accident)	
Mean	0.0411	
Standard deviation	0.1985	
Firing rate (from $t - 1$ to $t$ )	0.0515* (0.0288)	-0.0293 (0.0322)
Firm fixed-effects	No	Yes
Number of firm-month observations	86,530	

Notes: (a) \*\*\*, \*\*, \* denotes significance at the 1%, 5%, and 10% level respectively. (b) All regressions (linear probability model) include additionally the following control variables: calendar month dummies, year dummies, the number of male blue-collar workers aged 25–49 in the firm (in logs), and the firm average of employees' age (and its square), tenure (and its square), experience (and its square), logarithm of daily wage (in €) of this group of workers. (c) Sample selection: male, blue-collar workers, aged 25–49 years and employed in firms (manufacturing sector) with on average at least 100 employees over the sample period from 2000-2006. (d) Robust standard errors in parentheses (adjusted for clustering on firms). (e) Source: Own Calculations, based on AUVA and ASSD.

the distinction between *moderate* and *severe* workplace accidents reflects the extent to which the reporting of such an accident is at discretion of the workers. The results suggest that the higher the firm's firing rate, the more reluctant their workers are to report a *moderate* workplace accident. We do not find such a pattern for *severe* workplace accidents. Hence, our findings are consistent with the idea that the reporting behavior is the driving force behind the pro-cyclicality of the incidence of workplace accidents. Our data do not allow to determine whether *moderate* workplace accidents are rather over-reported in booms or under-reported in recessions. The results simply tell that *moderate* workplace accidents are *relatively* less reported in recessions. To the extent that our model captures the main aspect that drive workers' reporting decisions and firms' workplace-safety investment decisions our evidence is consistent with the idea that recessions are bad for workplace safety due to underinvestment in accident prevention.

## 5.5 Conclusions

Workplace accidents are related to workplace safety. Nevertheless, cyclical fluctuations in workplace accidents are puzzling from an economic point of view. Workplace accidents could be pro-cyclical because effort and hours of work are negatively related to unemployment and because higher work effort makes accidents more likely. Hence we should see more accidents during a boom and less during a recession. Alternatively, cyclical fluctuations in workplace accidents may be related to reporting behavior. In times of high unemployment workers are reluctant to report workplace accidents because they fear that employers will hold this against them. In this study we investigate this alternative explanation using high-quality Austrian matched worker-firm data containing information about workplace accidents of blue-collar workers in manufacturing. We find that workers who reported an accident in particular period of time are more likely to be fired later on. Apparently, when deciding about whom to fire, employers take the accident history of workers into account. Moreover, we find support for the idea that recessions affect the reporting of workplace accidents: if the probability to be dismissed is high, worker are less likely to report a moderate accident. For severe accidents we do not find such an effect.

The cyclical sensitivity of the incidence of workplace accidents appears to be related to reporting behavior. As indicated in the theoretical part of the chapter the cyclical fluctuations in reporting behavior has clear welfare implications as investments in prevention of workplace accidents may be suboptimal. If in recessions firing rates go up workers may underreport workplace accidents and thus firms under-invest in workplace safety. In booms workers may over-report workplace accidents and therefore firms over-invest in workplace safety, i.e. although workers benefit from the investments in workplace safety they are wasteful from a

social point of view.

In light of our theoretical analysis, our empirical evidence suggests that recessions are bad for workplace safety. From the point of view of economic policy, a way to bring the economy closer to the social optimum would be to introduce measures that impede the discrimination in firing against workers who reported an accident. This would increase the incentive of firms to invest in workplace safety also during recessions.

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## Curriculum Vitae

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Oct 2001 – Nov 2006	Studies in Economics at the University of Zurich and Lausanne
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